# ESSAYS ON EXPERIMENTS EXAMINING INCOMPLETE CONTRACTING AND COMMUNICATION

by

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As members of the Dissertation Committee, we certify that we have read the dissertation prepared by Sean Inoue titled Essays on Experiments Examining Incomplete Contracting and Communication and recommend that it be accepted as fulfilling the dissertation requirement for the Degree of Doctor of Philosophy.

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Final approval and acceptance of this dissertation is contingent upon the candidate's submission of the final copies of the dissertation to the Graduate College.

I hereby certify that I have read this dissertation prepared under my direction and recommend that it be accepted as fulfilling the dissertation requirement.

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# Dedication

This work is dedicated to my Grandfather, Shinya Inoue, whose dedication to his work as a Professor of Biology inspired me to follow in his footsteps. Thank you for everything.

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# Abstract

This manuscript explores how communication and commitment through a potentially incomplete contract impact decisions that people make. The first chapter in my dissertation explores the two problems simultaneously. The second chapter in my dissertation explores the impact that communication has using a cognitive hierarchy model, and the third chapter of my dissertation explores how players commit themselves when another person decides what happens in the cases that are not committed to. Across these projects, the two players playing have some conflict. When writing contracts there is a critical tension: If a person commits too much, they leave no room for flexibility. If a person commits too little, the other party will take advantage. Understanding how people weigh this key tension, understanding how partial commitment impacts communication, and understanding how communication impacts play are the goals of this dissertation.

The first and second chapter make it clear that the setting of the game impacts how communication occurs. In experiments, a written contract changes the effectiveness of communication among subjects. An outguessing game causes messages to have more meaning than standard game theory would predict. The first and the third chapter find that players make different choices when the conflict of interest between the two players changes. I additionally find a strong behavioral influence of communication in my second chapter. These results, along with many others, are a deep exploration into how people write contracts and communication in a variety of settings.

# Chapter 1

# An Experimental Analysis of Private Information in Constrained Contracting

I experimentally test a model of how organizations write rules when control over the action is limited and there is asymmetric information. A principal (writer) writes a rule that dictates an action for an agent (receiver) to take. The action is based on the information that the principal receives. The principal has an incentive to shape how the information is used, but has limited control because of the complexity both of the information they receive and of describing the action. If a principal does not retain control over the action for some information they may receive, the principal privately observes the information and has a chance to communicate to the agent. The principal has the core problem of how to optimally exercise limited control. In addition, the form of limited control may impact communication. This paper experimentally tests how rules are written and how complete rules may be. It is predicted that as preferences become more aligned between the principal and the agent, there is more scope for communication. Experimentally, this prediction is supported. Also, many subjects choose not to write a contract, even though it is theoretically optimal to do so. It is hypothesized that this is due to communication providing high average payoffs after no contract has been written. Subjects additionally fail to write rules that divide the information into two different categories to facilitate clearer communication.

**JEL**: C90, D23, D71, D86, K12

**Keywords**: *strategic communication, cheap talk, incomplete contracts, experimental economics*

## 1.1 Introduction

Heads of organizations have a complex and challenging task at hand when deciding upon rules (contracts) that govern the actions that must be taken. A principal who writes a rule that determines the action for an agent to take must decide the appropriate events to include in the contract, along with which actions to take in cases of those events, and carefully describe each event and action. In organizations, the information that a principal may observe and the actions that an agent may need to take can be complex to describe. The principal must then carefully decide the amount of detail to include in the rule, since it is impossible for the principal to write a completely detailed contract. Furthermore, the decision of what to include depends on the nature of what happens outside of clauses in the rule. Because only the principal observes all relevant information, states that fall outside of the rule are subject to communication between the principal and the agent. The principal must deduce how to optimally exercise limited control when the alternative to their control is communicating to the agent.

This experiment tests how writers write rules when they face this problem of limited control. In the experiment, a writer will, prior to observing the state of the world, write a contract. A “contract" will indicate three things: a low state, a high state, and a writer action. The contract stipulates that if a state is drawn between the low state and the high state (inclusive), the writer action will be taken. After the contract is written, a state is randomly drawn. If the state is lower than the low state or higher than the high state, the writer will privately observe the state. After observing the state, the writer will send a message to the receiver (acting as an interpreting party), who observes only the message sent and the contract written. If the receiver receives a message, the receiver will take a receiver action and the game ends. Because the receiver's ideal action given the state is different than the writer's ideal action given the state, there is a conflict of interest between the two players.

The writer has a difficult optimization problem: The writer can write a contract that fixes the action for more states, or the writer can rely more heavily on communication. Additionally, the writer can write a contract to induce different types of communication. At one extreme, the writer can write no contract and rely only on communication. After no contract has been written, the game looks similar to the model of communication presented in Crawford & Sobel (1982). At the other extreme, the writer can write a complete contract that stipulates a single action to be taken in every state of the world. This makes the action deterministic, meaning that the action has no sensitivity to the state. For no conflict of interest between the writer and receiver, only relying on communication is optimal, while for a large enough conflict of interest, a complete contract is optimal. For intermediate conflicts of interest, it is optimal for the writer to write partially complete contracts. A treatment with low conflict of interest and a treatment with high conflict of interest are utilized to test whether the completeness of a contract is sensitive to the preferences of the receiver. Experimental evidence supports that subjects write more complete contracts when their interests are less aligned.

However, subjects do not behave in accordance with other predictions. Given the states included in the contract, writers tend to include actions that are far from optimal. Writers also tended to write contracts that covered too many states in the low-conflict treatment and too few states in the high-conflict treatment. It is predicted that contracts should be written such that the writer can clearly communicate when a state is above or below the contracted region, but over half of the time, contracts are not written in this fashion. 30.2% of writers in the low bias treatment and 26.7% of writers in the high bias treatment chose to not write a contract and instead rely solely on communication. According to predictions, writers should always choose to write a contract.

One critical reason that some writers utilized full communication involves what happens when players reach the communication stage: payoffs are higher than predicted when the writer chooses not to write a contract. This is primarily due to messages being overly indicative of the state. On the other hand, there is undercommunication when the contract splits the remaining states into two separate regions, which leads to poor outcomes for both players. In addition, there was a small learning effect in both treatments as the number of states covered in the contract shrinks over time. I postulate that this is due to the poor performance of contracts with many states. However, there is not a clear causal link between the interpretation rule and the writing of the contract. It is unclear whether communication had an impact on the writing of contracts, or whether the specific writing of the contracts impacted communication. This remains to be the subject of future work.

In addition to the sessions in the US, I also ran experiments in Japan at Waseda University. I find that the play in the Japanese sessions more closely matched theory: errors in writer actions were smaller in magnitude and fewer, and the number of states covered in the contract was more different between treatments when compared to the Arizona group. However, the number of states included was similar in each treatment between Arizona and Japan. Many people (although fewer in the high bias Japan sessions) also chose not to write contracts. I postulate that some of these differences have to do with the high difference in sophistication of the subject pool. Because writing an action in a contract is a complex math problem, is it unsurprising that a subject pool with higher average CRT scores performed better.

There is experimental work that analyzes how subjects communicate. Cai & Wang (2006) Cai Wang experimentally test the results of the theoretical framework of Crawford & Sobel (1982). That experiment shows that senders (writers) tend to overcommunicate information and that receivers tend to believe said information is true. In addition, payoffs tend to be close to what is predicted in the theory of Crawford & Sobel. This overcommunication result is confirmed in Wang et. al. (2010). In this paper, analyzing communication subgames after a contract has been written shows that there are both communication subgames in which payoffs were better than predicted and communication subgames in which payoffs were worse than predicted. This paper adds to the literature by analyzing communication with different restrictions on the states. Unlike the result in Cai & Wang that shows only overcommunication, this paper shows that subjects overcommunicate in some communication subgames and undercommunicate in others. Wang et. al. (2010) use eye-tracking software to show that senders view the game in a way that is consistent with level-k, indicating that may be a way to predict how subjects communicate in this paper. This overcommunication result is additionally highlighted in Blume et. al. (2001), who in one experiment find that subjects are overly truthful about their type when they should not be.

There is a large body of theoretical work focused on incomplete contracts with restrictions on the complexity of a contract. In these settings, it is sometimes optimal to intentionally write incomplete contracts. Dye (1982) is one of the first models in this literature, detailing how contractual incompleteness can come about in markets. Simon (1991) identifies how contractual incompleteness arises in employer/employee relationships. Shavell (2006) analyzes the role of interpretation in contracts, and solves for which interpretive rules are optimal in an incomplete contracting framework. Heller & Spiegler (2008) add to this framework by allowing for contradictory statements to exist in a contract. In these detailed theoretical models, the authors are interested in which contracts written by the writers maximize the writers' payoff in equilibrium, as well as the optimal interpretive rule chosen by the party interpreting gaps or contradictions. In each of these papers, there are common themes regarding the way in which the contract is written. For example, as conflict of interest increases, those papers and that model predict contracts to be more complete. This paper experimentally tests whether human subjects write contracts in accordance with those common themes.

Previous work has explored how varying the setting impacts how agents write contracts. Fehr & Schmidt (2007) analyze how fairness impacts contract design. Brandts, Charness, & Ellman (2012) analyze how communication affects the design of a contract, which is a key question of this paper. However, the setting of this paper focuses on communication after a contract has been written as an interpretive rule instead of focusing on the impact of ex-ante, free-form communication in forming agreements.

The remainder of the paper will proceed as follows: Section two provides an overview of the experimental setup and characterizes the set of optimal contracts, as well as describing the set of predictions tested in the experiment. Section three discusses the experimental design. Section four analyzes the experimental results and the sessions with subjects at Waseda University in Japan. Section five concludes the paper.

## 1.2 Experimental Setup

### 1.2.1 Description

There are two agents: a writer and a receiver. In the game $G$, a writer will be writing a contract (called a "rule" in the experiment) that dictates an action to be taken in certain states, while the receiver will be providing interpretations for messages the writer sends when the state is outside of the contract. The writer has payoff $U_W(s, a; b) = 30 - |s + b - a|^{1.4}$, while the receiver has payoff $U_R(s, a) = 30 - |s - a|^{1.4}$, where $s \in S = \{1, 3, 5, 7, 9\}$ is a state drawn from a uniform distribution over the state space $S$, $a \in \{\mathbb{R} \bmod 0.25\}$ is the action taken, and $b \geq 1$ is the bias term.[1] Note that, given the state is common knowledge, if the action were continuous, the optimal writer action would be $a_W^* = s + b$ while the optimal receiver action is $a_R^* = s$.

In stage one, the writer writes a contract (rule) $C = (s_{low}, s_{high}, a_W)$ that indicates a low state $s_{low}$, a high state $s_{high}$, and a writer action $a_W$. Informally, the contract states that when a $s$ is drawn that is between the low state and the high state or equal to either of the two states, the writer action is taken. The writer is allowed to write no contract, in which case $C = \emptyset$.

In stage two, the state $s \in S = \{1, 3, 5, 7, 9\}$ is drawn from a uniform distribution over $S$. If $s_{low} \leq s \leq s_{high}$, then the action $a = a_W$ is taken and the game terminates. If $s < s_{low}$ or $s > s_{high}$, the writer privately observes $s$ and the game proceeds to stage 3.

In stage three, if the game has not ended, the writer sends a costless message $m \in \{0, 1, ..., 9, 10\}$ to the receiver.[2]

In stage four, the receiver observes the message and then takes an action $a_R$, which causes the game to end. Actions are allowed to be multiples of 0.25. When the game ends, the writer and receiver both realize payoffs and observe the state, the action taken, and the contract.

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[1] I use $b > 1$ instead of $b > 0$ because this eliminates almost all possible Bayesian Nash equilibria, in which states inside one equilibrium partition element play a mixed strategy that mixes between messages sent with probability 1 in other partition elements. These equilibria arise due to the discreteness of the state space, so using $b \geq 1$ simplifies the analysis significantly while preserving the application of the analysis to the experiment

[2] This includes messages 0 and 10 in case the writer wants to exaggerate about the state.

To introduce some terminology, a *gap* is be defined as the set of states not covered by the contract, $\{s < s_{low}\} \cup \{s > s_{high}\} = \mathcal{G}$. A *lacuna* is defined as either $\{s < s_{low}\}$ or $\{s > s_{high}\}$. A contract is considered complete if there is no gap ($C = (1, 9, a_W)$) and otherwise is considered incomplete.

A communication subgame, $\Gamma^C$, is defined as a game in which a state is drawn from a uniform distribution over $S^C = \{s \notin \{s_{low}, \ldots, s_{high}\}\} \subseteq S$ that the writer privately observes. The writer then sends a message $m \in M$ to the receiver. After the receiver receives a message, they take an action $a_R$. Note that $\Gamma^C$ only exists if $C \neq (1, 9, a_W)$. At $\Gamma^C$, a strategy for the writer $\sigma_W^C : \mathcal{G} \to \Delta(M)$ maps from the gap into distributions over the message space. A strategy for the receiver $\sigma_R^C : M \to \Delta(\mathbb{R})$ maps from the messages into distributions over the action space. In the overall game $G$, a strategy for the writer $\left(C, \left(\sigma_W^{C'}\right)_{C' \in \mathbf{C}}\right)$ is a contract and a strategy for the writer within each communication subgame. A strategy for the receiver $\left(\sigma_R^{C'}\right)_{C' \in \mathbf{C}}$ is a strategy for the receiver within each communication subgame. This paper is concerned with optimal contracts, where an optimal contract is defined as the contract that yields the highest payoff to the writer in any pure-strategy perfect Bayesian equilibrium. The next section will formally characterize optimal contracts.

### 1.2.2 Observations

This section outlines properties of optimal contracts. These properties will be utilized to make predictions about the experimental results. The experimental test will use two treatments: $b = 1.25$ and $b = 2.25$. In each of these treatments, contracts are predicted to be incomplete in different ways. These two treatments will be used to test the primary predictions of the model. This section begin with some observations that will narrow the range of possible equilibria. Next, the section will give a full characterization of optimal contracts for all $b \geq 1$. Following this will be notes on properties that will be of interest when making predictions about play in the following section.

The first observation is a characterization of the best possible writer action. It additionally characterizes the best possible receiver action if the receiver knows the states over which a message is sent. The action is pinned down by the structure of the payoffs combined with the uniformly distributed states.

**Observation 1.** *Given a low state $s_{low}$ and a high state $s_{high}$, the optimal contract specifies the writer action $a_w = \frac{s_{low}+s_{high}}{2} + b$. Conditional on the receiver knowing the state $s \in S' \subseteq S$, the optimal receiver action is $\mathbb{E}[s \mid s \in S']$.*

It is also possible to write down the structure of perfect Bayesian equilibria in any communication subgame. After all possible equilibria are found, optimal contracts will be found by finding sender-optimal equilibria and comparing contracts given that the equilibrium is sender optimal.

**Observation 2.** *In any communication subgame, a perfect Bayesian equilibrium of that subgame is represented as a partition $P = \{p_1, \ldots, p_n\}$ for $n \geq 1$, where $p_i = \left\{s_1^i, ..., s_{k_i}^i\right\}$ is a partition element. Any message $m \in M_i \subseteq M$ that is sent for $s^i \in p_i$ induces a unique expected action $a_R^i \neq a_R^j \forall j$, where $a_R^i$ is the expected receiver preferred action given $m \in M_i$ is sent. Each partition element is a convex set in $S$ and ordered such that for any $s^i \in p_i$, $s^j \in p^j$, $s^i < s^j$.* [3]

**Observation 3.** *A writer will always choose to write a contract.*

The key is that, in any communication equilibrium, getting rid of the lowest partition element does not change the remainder of the states being in equilibrium. If a contract is inserted where the lowest partition element is, a strict payoff improvement can be gained by the writer, who now gets their preferred action in that region of the state space.

The next two observations fully outline a characterization of the optimal contract. The optimal contract for any $b$ will be shown in table 1.1. The details of this calculation will be shown in Appendix C.

**Observation 4.** *The optimal contract for any $b > 1$ is found by first computing all writer-optimal perfect Bayesian equilibria within each communication subgame. Given that any communication subgame will contain an equilibrium that maximizes the writers payoff, the optimal contract is the contract that selects a communication subgame in a way that maximizes the writer's payoff. The optimal contract for all $b > 1$ is displayed in figure 1.1.*

[3] Using the weak topology over S

**Observation 5.** *The number of states specified in the optimal contract weakly decreases as the bias decreases.*

Table 1.1: Optimal Contracts

| Range of $b$ | Optimal Contract(s) $(s_{low}, s_{high}, a_W)$ | Expected Payoff to Writer | Expected Payoff to Receiver |
|---|---|---|---|
| $b = 1$ | $(s, s, s+b)\ \forall s \in \{1,3,5,7,9\}$ | $30 - \frac{4\lvert b\rvert^{1.4}}{5}$ | $30 - \frac{\lvert b\rvert^{1.4}}{5}$ |
| $1 < b \leq 1.5$ | $(3,3,3+b)$,$(7,7,7+b)$ | $30 - \frac{2\lvert b\rvert^{1.4}+\lvert b-1\rvert^{1.4}+\lvert b+1\rvert^{1.4}}{5}$ | $30 - \frac{\lvert b\rvert^{1.4}+2}{5}$ |
| $1.5 < b \leq 2.149$ | $(3,5,4+b)$,$(5,7,6+b)$ | $30 - \frac{2+\lvert b\rvert^{1.4}+\lvert b-1\rvert^{1.4}+\lvert b+1\rvert^{1.4}}{5}$ | $30 - \frac{\lvert b+1\rvert^{1.4}+\lvert b-1\rvert^{1.4}+2}{5}$ |
| $2.149 < b \leq 3.610$ | $(3,7,5+b)$ | $30 - \frac{2\lvert 2\rvert^{1.4}+2\lvert b\rvert^{1.4}}{5}$ | $30 - \frac{\lvert b+2\rvert^{1.4}+\lvert b\rvert^{1.4}+\lvert b-2\rvert^{1.4}}{5}$ |
| $3.610 < b \leq 4.375$ | $(1,7,4+b)$,$(3,9,6+b)$ | $30 - \frac{2\lvert 3\rvert^{1.4}+2+\lvert b\rvert^{1.4}}{5}$ | $30 - \frac{\lvert 3+b\rvert^{1.4}+\lvert 1+b\rvert^{1.4}+\lvert 1-b\rvert^{1.4}+\lvert 3-b\rvert^{1.4}}{5}$ |
| $b \geq 4.375$ | $(1,9,5+b)$ | $26.159$ | $30 - \frac{\lvert 4+b\rvert^{1.4}+\lvert 2+b\rvert^{1.4}+\lvert b\rvert^{1.4}+\lvert 2-b\rvert^{1.4}+\lvert 4-b\rvert^{1.4}}{5}$ |

### 1.2.3 Predictions

1. Writers will always write contracts.
2. Each gap, if it exists, will be such that communication is utilized on either side of the contract. That is, $s_{low} > 1$ and $s_{high} < 9$.
3. The contract that the writer writes is such that $a_W = \frac{s_{low}+s_{high}}{2} + b$.
4. As $b$ increases from 1.25 to 2.25, the number of states covered by a contract will increase.
5. In any communication subgame, communication will be consistent with the most informative perfect Bayesian Nash equilibrium.
6. Writers are more likely to write different contracts after receiving a bad payoff.

While predictions 1-4 have to do with the contract writing, prediction 5 analyzes behavior within communication subgames. In an equilibrium, given contracts written by writers, communication should be optimal. Given behavior within communication subgames, writers will choose the contract that gives them the best payoff. If prediction 5 is violated, communication is not in equilibrium. If prediction 6 holds, then writers are sensitive to negative payoff shocks and are attempting to select contracts based on the payoffs they earn.

## 1.3 Experimental Design

Subjects completed the task at the Experimental Science Laboratory at the University of Arizona. The experiment was coded in z-Tree. Subjects participated in a total of eight sessions in the US–four for each treatment. The $b = 1.25$ treatment had a total of 54 subjects–27 writers and 27 receivers. The $b = 2.25$ treatment had a total of 58 subjects–29 writers and 29 receivers. Each session had between 8 and 18 people. Within each session, each subject played two practice rounds of the game. In each practice round, subjects played by themselves and made the decisions of both roles. At the end of those practice rounds, each subject was quizzed on the results. These quizzes had no payout implications, but subjects were encouraged to ask questions if they got the quiz wrong. Following the quiz phase, subjects played 30 rounds, with roles fixed as either the writer or the receiver across all rounds. Matching was done randomly. At the end of the experiment, subjects were paid for two of the thirty rounds chosen randomly. Each subject earned 33 cents per ECU, along with a show-up fee of $10. The experiment took between 105 minutes and 150 minutes. Subjects were paid an average of $27.27. The only difference between the setup above and the experiment is that subjects were restricted to actions that were a multiple of 0.25 that were between 1 and 12.

In addition, as a robustness check, instructions and the z-Tree file were translated into Japanese by a Waseda University graduate student. Subjects participated in two sessions at the Experimental Science Laboratory at Waseda University in Japan with the help of Waseda faculty and graduate students. There was one session for each treatment. The $b = 1.25$ treatment had 22 total subjects–11 in each role–and the $b = 2.25$ treatment had 20 total subjects, with 10 in each role. Each subject was paid 37 yen per ECU, along with a 1000 yen show up fee. Each session took two hours. Subjects were paid an average of 2995 yen.

## 1.4 Experimental Results

### 1.4.1 How People Write Contracts

This section discusses whether subjects played in accordance to predictions 1-4. These predictions focus on how writers write contracts without detailed analysis on how communication might impact results. A summary of the total number of contracts written of each type is presented in table 1.2

**Result 1.** Prediction one is supported. Across both treatments, subjects commonly chose to not write contracts.

For result one, table 1.3 shows the fraction of periods in which no contract was written, while table 1.4 shows the fraction of subjects who chose not to write a contract in at least 10/20 periods. Over all, roughly 30% of periods in the $b = 1.25$ treatment and roughly 25% of periods in the $b = 2.25$ treatment had no contract

Table 1.2: Events that Writers Wrote in Contracts

| (Low State, High State) | b=1.25 | | b=2.25 | |
|---|---|---|---|---|
| | Total Instances | Number of Unique Subjects | Total Instances | Number of Unique Subjects |
| No Contract | 245 | 16 | 232 | 14 |
| (1,1) | 25 | 4 | 12 | 4 |
| (3,3) | 19 | 6 | 31 | 6 |
| (5,5) | 21 | 6 | 37 | 8 |
| (7,7) | 5 | 4 | 15 | 4 |
| (9,9) | 34 | 5 | 48 | 7 |
| (1,3) | 94 | 13 | 37 | 7 |
| (3,5) | 72 | 11 | 20 | 8 |
| (5,7) | 18 | 9 | 39 | 11 |
| (7,9) | 14 | 6 | 59 | 9 |
| (1,5) | 30 | 8 | 53 | 7 |
| (3,7) | 103 | 15 | 85 | 13 |
| (5,9) | 8 | 5 | 45 | 9 |
| (1,7) | 41 | 11 | 11 | 5 |
| (3,9) | 46 | 11 | 25 | 6 |
| (1,9) | 35 | 10 | 121 | 11 |

Table 1.3: What Fraction of Writers did not Write Contracts

| Periods | US b=1.25 | US b=2.25 | Predicted |
|---|---|---|---|
| 1-5 | 0.222 | 0.172 | 0 |
| 6-10 | 0.267 | 0.269 | 0 |
| 11-15 | 0.311 | 0.290 | 0 |
| 16-20 | 0.326 | 0.303 | 0 |
| 21-25 | 0.356 | 0.283 | 0 |
| 26-30 | 0.333 | 0.283 | 0 |
| Overall | 0.302 | 0.267 | 0 |

Table 1.4: What Fraction of Writers did not Write Contracts in at Least 10/20 of the 30 Periods Played

| Minimum Number of Periods | b=1.25 (N=27) | b=2.25 (N=29) |
|---|---|---|
| 10 | 0.333 | 0.310 |
| 20 | 0.259 | 0.241 |

written. Over time, the number of subjects abstaining from writing a contract increases. In addition, looking at subject-level data, roughly a third of subjects wrote no contract a third of the time in each treatment, while roughly a quarter of subjects wrote no contract two-thirds of the time in each treatment. Given that there are a significant portion of subjects choosing to not write contracts, and given that learning seems to go in the opposite direction of the prediction, prediction one can be rejected.

**Result 2.** Prediction two is not supported. Across both treatments, subjects commonly choose to write a contract that included either $s_{low} = 1$ or $s_{high} = 9$.

As can be seen in figure 1.2, when restricting the sample to only cases where a writer writes a contract, it includes the states 1 or 9 57.9% of the time in the b=1.25 treatment and 64.4% of the time in the b=2.25 treatment. Thus, this prediction is strongly rejected.

**Result 3.** Prediction three is not supported. Subjects are classified as having approximately correct actions given their contracts as long as the action in the contract is within $\pm 1$ of the correct action given the contract. This classification only captures 51.24% of the data in the $b = 1.25$ treatment and 70.95% of the data in the $b = 2.25$ treatment. In addition, when using a rank-sum test to see whether the distributions of writer action errors are the same across both treatments, the hypothesis that the two groups of subjects have similar distributions of writer action errors relative to the contract written is rejected at the 1% level.

The total distribution of errors can be seen in figure 1.1. The average absolute error in the $b = 1.25$ treatment was 1.918, while the average absolute error in the $b = 2.25$ treatment was 1.046. A rank-sum test for a difference in means yielded a

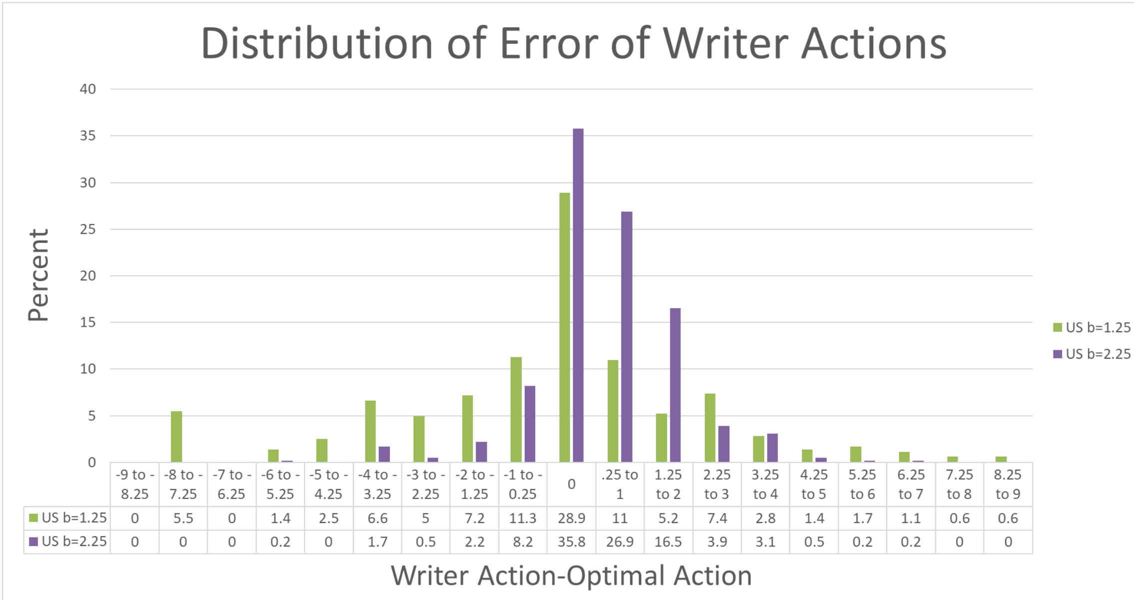



Figure 1.1: Difference Between the Optimal Writer Action and the Actual Writer Action Given the Contract (Excluding First 10 Periods)

p-value of $5.268 \times 10^{-63}$, so I can reject the null hypothesis that players make the same errors in the two treatments.

This result that can be explained in one of two ways: Either writers have preferences that are biased towards their receiving partners, or this particular part of the task is heavily influenced by the sophistication of the subjects. Looking at the distribution, many of the errors are in the positive direction, indicating that the is not a result of preferences such as guilt or positive reciprocity, which negative errors might indicate.

When comparing the two treatments, the distributions of differences between actual action and predicted action are different at the 1% level using a rank-sum test. This indicates a difference between how subjects were thinking about the problem between the two treatments. Why this may be the case is an important question. It is possible that, once again, this has to do with sophistication, as this difference disappears in the Japan treatment.

**Result 4.** Prediction four is supported. Using a rank-sum test, for all data, the

Table 1.5: Average Number of States in Contract

| | b=1.25 | | b=2.25 | | |
|---|---|---|---|---|---|
| Periods | N | Mean | N | Mean | p-value |
| 1-5 | 135 | 2.356 (0.142) | 145 | 2.497 (0.137) | 0.23348 |
| 6-10 | 135 | 1.963 (0.132) | 145 | 2.007 (0.144) | 0.51443 |
| 11-15 | 135 | 1.815 (0.131) | 145 | 2.021 (0.142) | 0.18419 |
| 16-20 | 135 | 1.637 (0.122) | 145 | 1.862 (0.140) | 0.20387 |
| 21-25* | 135 | 1.474 (0.119) | 145 | 1.779 (0.132) | 0.08651 |
| 26-30** | 135 | 1.467 (0.119) | 145 | 1.910 (0.141) | 0.02714 |
| All Periods*** | 810 | 1.785 (0.184) | 870 | 2.013 (0.154) | 0.00974 |

*, **, and *** indicate a significant difference between means at the 10%, 5%, and 1% levels using a Mann-Whitney U (rank-sum) test. Standard errors are in parenthesis.

distribution of the number of states included in contract is different at the 1% level. In addition, periods 26-30 differ at the 5% level, while periods 21-25 differ at the 10% level.

The average number of states included in the contract starts off at a similar point and diverges after many periods of play. However, as is shown in the results of figure 1.5, using a rank-sum test for each group of 5 periods yields a significant difference for the last two periods in the sample. In addition, if all periods of both treatments are compared, there is a difference at the 1% level. This evidence is not the strongest, as the rank-sum test treats each individual period as a separate data point, ignoring correlation by individuals. In addition, there is no significance at the one-period level. However, because learning goes in the correct direction, and because there is significant difference in the aggregate and in the last set of five periods, this constitutes evidence in support of prediction four.

Looking at the data, it is clear that the number of states included in the contract is far from what is predicted in the analysis on optimal contracts. Interestingly, the two treatments seem to have errors in opposite directions: In the low-bias treatment, more states are included on average than what is predicted. In the high-bias treatment, fewer states are included on average than what is predicted.

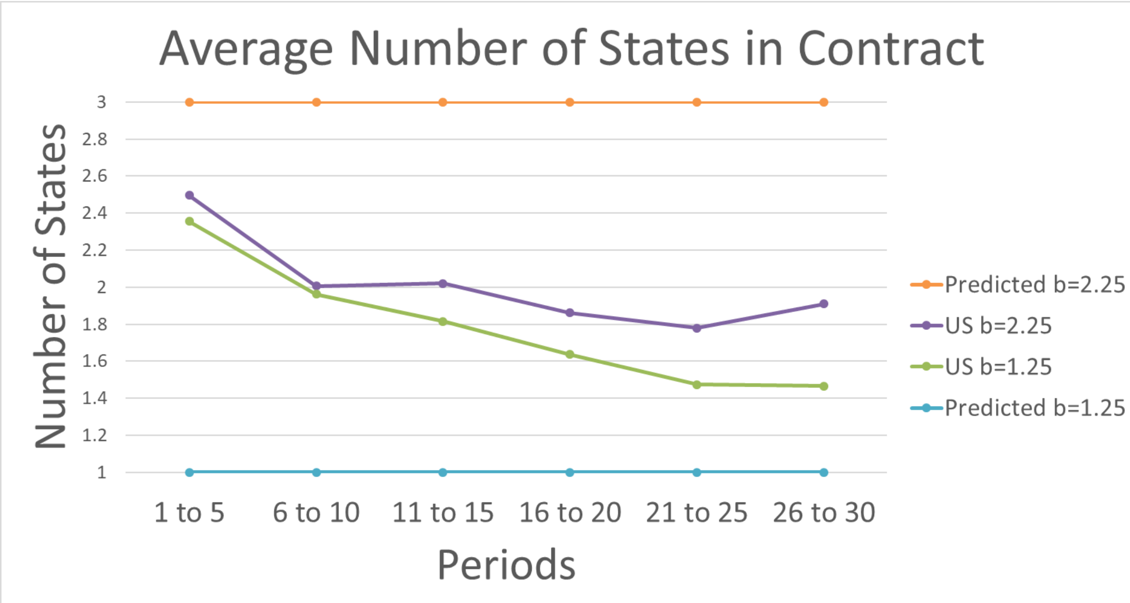



Figure 1.2: Average Number of States in Contract

### 1.4.2 Behavior Within Communication Subgames and Impacts on Contract Writing

The goal of this section will be to provide some insight as to why writers do not write contracts like the theory predicted. The main result of this section is that, in many communication subgames, the actual average payoff is significantly higher than predicted. In addition, compounded with the error that comes with subjects writing actions in contracts, subjects who relied on more communication tended to do better in both treatments.

In looking at prediction five, I analyzed communication subgames which were reached by many subjects and had many data points. For many of the contracts, the sample size was too small to make any meaningful predictions, so I chose communication subgames that were reached regularly and by many different subjects. Unfortunately, only six unique subjects wrote a contract with $s_{low} = s_{high} = 3$, with four unique subjects writing the contract with $s_{low} = s_{high} = 7$, so analysis would not be meaningful for those communication subgames.

**Result 5.** Prediction five is not supported. Overall, play is not consistent with the most informative perfect Bayesian Nash equilibrium.

This result was obtained with the assumption that writers randomize uniformly over any messages that are numbers contained within a partition element within a communication subgame. When analyzing many communication subgames with sufficiently high data, the state-message, message-action, and state-action correlations are statistically different than the most informative communication equilibrium using a t-test for differences in correlation.[4] This is not true of all communication subgames analyzed in the paper. Notably, the communication subgame after $s_{low} = 5$, $s_{high} = 7$ for $b = 2.25$ has communication that is not significantly different than the most informative perfect Bayesian Nash equilibrium. However, the fact that this occurs for only one of the two treatments still supports the conclusion that overall play is significantly different from predicted.

When looking at table 1.6, note that it is not true that writers always overcommunicate and that receivers overly believe the writer. In many communication subgames, there is more muddled communication than is predicted by the model, or else the receiver believes the writer less than what is predicted. This is particularly harmful in communication subgames like the one that occurs after the contract $C = \{3, 7, a_W\}$, where there should be completely honest communication. However, what happens is far from honest, as there is only a .448 correlation between state and action in the $b = 1.25$ treatment and a .690 correlation in the $b = 2.25$ treatment, both of which are statistically lower than what is predicted using a t-test. This is contrary to a well-known result in Cai & Wang (2006), where the authors find that overcommunication is common.

[4]Following Cai & Wang, the regression $Y = \alpha + (r_{XY} + \beta)\, X + \epsilon$ was run, where $r_{XY} = (s_Y/s_X)\, \sigma_{XY}$, $s_X$ and $s_Y$ are the sample standard deviations of $X$ and $Y$ and $\sigma_{XY}$ is the theoretical correlation. The $t$-test on $\beta$ would say whether the actual correlation $Corr(X, Y)$ is statistically different from the theoretical correlation $\sigma_{XY}$. The regressions were run with $(Y, X)$ being one of (State, Message), (Message, Action), and (State, Action). This was done for many different communication subgames, such that there were many different subjects and data points. Some with fewer subjects/data points were also included to try to cover more communication subgames.

Table 1.6: Correlations in Selected Communication Subgames

| Communication Subgame Following the Contract (Low State, High State) | b | # of Times Communication Subgame is Reached. | Predicted State-Message | Actual State-Message | Predicted Message-Action | Actual Message-Action | Predicted State-Action | Actual State-Action |
|---|---|---|---|---|---|---|---|---|
| No Contract | 1.25 | 245 | 0.776 | 0.848$^{**}$ | 0.896 | 0.792$^{***}$ | 0.866 | 0.674$^{***}$ |
| No Contract | 2.25 | 232 | 0.527 | 0.643$^{**}$ | 0.745 | 0.751 | 0.707 | 0.555$^{***}$ |
| (5,5) | 2.25 | 29 | 0.915 | 0.894 | 0.965 | 0.802 | 0.949 | 0.694$^{*}$ |
| (1,3) | 1.25 | 55 | 0.783 | 0.495$^{**}$ | 0.905 | 0.401$^{***}$ | 0.866 | 0.416$^{***}$ |
| (1,3) | 2.25 | 25 | 0 | 0.605$^{***}$ | 0 | 0.598$^{***}$ | 0 | 0.299$^{*}$ |
| (3,5) | 1.25 | 55 | 0.951 | 0.560$^{***}$ | 0.980 | 0.611$^{***}$ | 0.971 | 0.315$^{***}$ |
| (5,7) | 1.25 | 15 | 0.952 | 0.381$^{**}$ | 0.980 | 0.531$^{**}$ | 0.971 | 0.368$^{**}$ |
| (5,7) | 2.25 | 21 | 0.952 | 0.944 | 0.980 | 0.830 | 0.971 | 0.748 |
| (3,7) | 1.25 | 56 | 1 | 0.624$^{***}$ | 1 | 0.755$^{***}$ | 1 | 0.448$^{***}$ |
| (3,7) | 2.25 | 36 | 1 | 0.791$^{**}$ | 1 | 0.821$^{**}$ | 1 | 0.690$^{***}$ |

$^{*}$, $^{**}$, and $^{***}$ indicate a significant difference from the predicted values at the 10%, 5%, and 1% levels using a t-test for differences in correlation

**Result 6.** Prediction six is supported. Subjects tended to switch contracts more often when they earned a poor payoff in the previous round.

The behavior inside communication subgames explains some of the behavior of writers in the previous subsection. Communication games where very incomplete contracts are written did better than communication in many other communication subgames, as can be observed in table 1.7. This means that writers were strongly incentivized to abstain from writing a contract. A key question is whether writers wrote contracts taking this into account or whether communication was influenced by the writing of the contract. Although the latter does not seem true, the former seems to have some merit due to the large number of people writing no contract. In table 1.8, it is evident that more subjects switch the contract they write after receiving a bad payoff, indicating that subjects are responsive to receiving bad payoffs.

Table 1.7: Writer Payoff Given Contract

| (Low State, High State) | b=1.25 $n$ | b=1.25 Average Payoff | b=1.25 Predicted Payoff | b=2.25 $n$ | b=2.25 Average Payoff | b=2.25 Predicted Payoff |
|---|---|---|---|---|---|---|
| No Contract | 245 | 27.4 | 26.438 | 232 | 26.066 | 20.938 |
| (1,1) | 25 | 26.063 | 27.15 | 12 | 25.483 | 21.938 |
| (3,3) | 19 | 23.975 | 28.35 | 31 | 24.907 | 24.35 |
| (5,5) | 21 | 28.103 | 27.95 | 37 | 25.800 | 25.15 |
| (7,7) | 5 | 29.19 | 28.35 | 15 | 26.732 | 24.15 |
| (9,9) | 34 | 24.752 | 27.15 | 48 | 28.139 | 21.95 |
| (1,3) | 94 | 26.928 | 28.265 | 37 | 25.015 | 24.962 |
| (3,5) | 72 | 24.862 | 28.265 | 20 | 25.110 | 26.162 |
| (5,7) | 18 | 24.363 | 28.265 | 39 | 27.173 | 26.162 |
| (7,9) | 14 | 27.653 | 28.265 | 59 | 28.231 | 24.962 |
| (1,5) | 30 | 27.146 | 27.375 | 53 | 25.927 | 25.975 |
| (3,7) | 103 | 25.076 | 27.775 | 85 | 26.755 | 26.375 |
| (5,9) | 8 | 26.781 | 27.375 | 45 | 26.438 | 25.975 |
| (1,7) | 41 | 23.703 | 25.6875 | 11 | 22.777 | 24.988 |
| (3,9) | 46 | 24.373 | 25.6875 | 25 | 24.532 | 24.988 |
| (1,9) | 35 | 24.716 | 22 | 121 | 25.450 | 22 |

Table 1.8: Did Writers Switch Contracts when Receiving Bad Payoffs

| Payoff Received in Previous Period | % Stayed with the Same Contract in the Following Period | |
|---|---|---|
| | $b = 1.25$ | $b = 2.25$ |
| $> 27$ ECUs | 69.2% (0.031) | 71.4% (0.024) |
| $\leq 27$ ECUs | 43.1% (0.020) | 64.3% (0.022) |
| t Statistic | 7.273*** | 2.190** |

*, **, and *** indicate a significant difference from the predicted values at the 10%, 5%, and 1% levels using a two sample t-test assuming equal variances. Standard errors are in parenthesis.

### 1.4.3 Robustness Check: Experimental Results of Sessions in Japan

In addition to the primary treatments, two sessions were run at Waseda University. This serves as a good check on whether the sophistication of subjects impacts the results of the experiment.

Because the sample size is too small, results regarding behavior in communication subgames are mainly useless, as each communication subgame is only reached by five or fewer total subjects. Thus, this subsection will focus primarily on how predictions 1-4 are impacted.

**Result 7.** The sophistication of subjects impacts how subjects play in the following way: Writer actions contain far less error and are similar in error across treatments. Fewer subjects chose to not write a contract in the b=2.25 treatment.

For prediction one, as can be seen in table 1.9, in the b=2.25 treatment, far fewer subjects chose not to write a contract, although the number is still statistically significantly different from the predicted amount, zero. For prediction three, distributions of action errors are smaller (average absolute errors of 0.715 in the $b = 1.25$ treatment and 0.656 in the $b = 2.25$ treatment) and are statistically different at the 10% level when using a rank-sum test (p-value of 0.08971). In the Japanese treatment, the errors tend to skew downwards in the direction of an inequality-averse writer who would be more likely to include actions that favor the receiver. In addition, as can be observed in figure 1.3, the errors have a much tighter distribution. For prediction four, in each group of five periods, the number of states included in a contract is significantly different at some level, which can be seen in table 1.11.

The key factor that likely influences the stark difference between how contracts are written in the two subject groups is subject sophistication. According to the director of the ESL at Waseda University, Yukihiko Funaki, the average CRT (Cognitive Reflection Test) score of the Waseda subject pool is 2.02. Charles Noussair, the director of the ESL at the University of Arizona, states that the average CRT score of the University of Arizona subjects is around 0.8. Calculating the correct action to use is mathematical in nature, and so a logical conclusion is that subjects with a higher CRT would do better at writing correct actions. In addition, there seemed to be a more powerful learning effect in the University of Arizona treatments than in

Table 1.9: What Fraction of Writers Did Not Write Contracts in Japan Sessions

| Periods | US b=1.25 | US b=2.25 | JPN b=1.25 | JPN b=2.25 | Predicted |
|---|---|---|---|---|---|
| 1-5 | 0.222 | 0.172 | 0.2 | 0.12 | 0 |
| 6-10 | 0.267 | 0.269 | 0.309 | 0.12 | 0 |
| 11-15 | 0.311 | 0.290 | 0.291 | 0.02 | 0 |
| 16-20 | 0.326 | 0.303 | 0.327 | 0.06 | 0 |
| 21-25 | 0.356 | 0.283 | 0.291 | 0.1 | 0 |
| 26-30 | 0.333 | 0.283 | 0.273 | 0.04 | 0 |
| Overall | 0.302 | 0.267 | 0.282 | 0.077 | 0 |

the Waseda treatments, indicating that in the Japanese treatments, subjects more quickly grasped ideas about how to write contracts.

State-message, message-action, and state-action correlations are also analyzed. These are found in table 1.12. For the small sample size that is available, communication after no contract was written is closer to the results of Cai & Wang, while communication with one state on each side is even worse than before, with a state-action correlation of 0.161 in the $b = 1.25$ treatment and a state-action correlation of 0.251 in the $b = 2.25$ treatment. This provides additional evidence that undercommunication may occur in communication subgames, and that undercommunication may influence behavior.

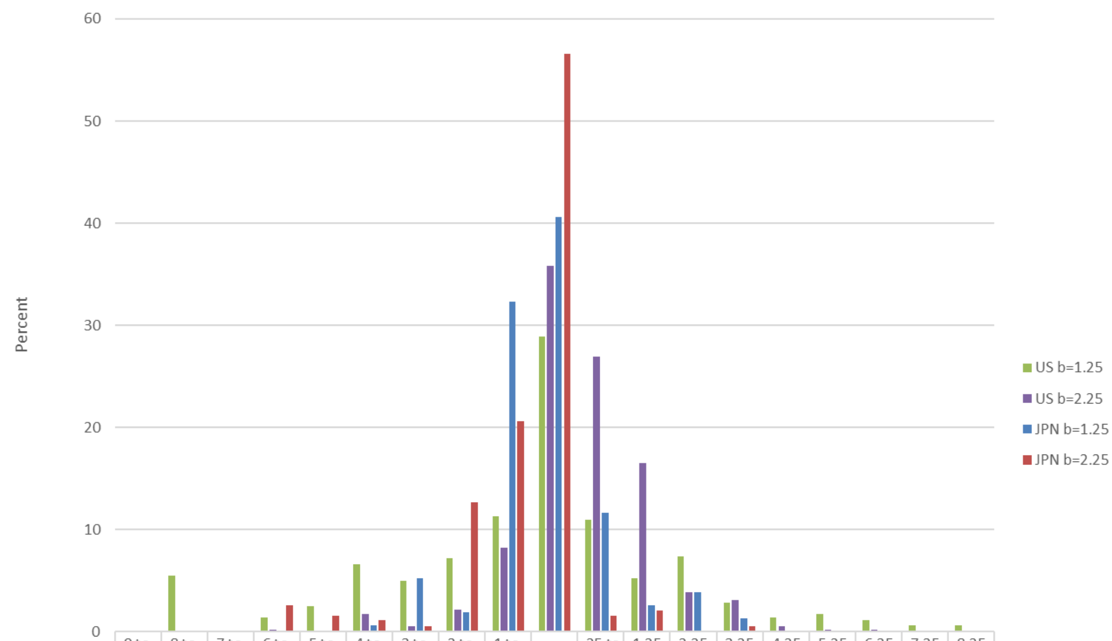
Table 1.10: Events that Writers Wrote in Contracts in Japan Sessions

| (Low State, High State) | b=1.25 | | b=2.25 | |
|---|---|---|---|---|
| | Total Instances | Number of Unique Subjects | Total Instances | Number of Unique Subjects |
| No Contract | 93 | 5 | 23 | 5 |
| (1,1) | 6 | 2 | 4 | 1 |
| (3,3) | 24 | 3 | 4 | 4 |
| (5,5) | 2 | 2 | 12 | 3 |
| (7,7) | 0 | 0 | 0 | 0 |
| (9,9) | 7 | 4 | 59 | 3 |
| (1,3) | 25 | 5 | 1 | 1 |
| (3,5) | 13 | 3 | 12 | 4 |
| (5,7) | 46 | 5 | 39 | 4 |
| (7,9) | 49 | 5 | 12 | 5 |
| (1,5) | 12 | 4 | 11 | 2 |
| (3,7) | 21 | 2 | 52 | 4 |
| (5,9) | 9 | 3 | 50 | 4 |
| (1,7) | 7 | 2 | 1 | 1 |
| (3,9) | 10 | 3 | 14 | 3 |
| (1,9) | 6 | 3 | 6 | 4 |

Table 1.11: Average Number of States in Contract in Japanese Sessions

| Periods | b=1.25 N | b=1.25 Mean | b=2.25 N | b=2.25 Mean | p-value |
|---|---|---|---|---|---|
| 1-5* | 55 | 1.927 (0.189) | 50 | 2.34 (0.199) | 0.05158 |
| 6-10*** | 55 | 1.509 (0.162) | 50 | 2.16 (0.177) | 0.00430 |
| 11-15*** | 55 | 1.582 (0.131) | 50 | 2.26 (0.139) | 0.00262 |
| 16-20** | 55 | 1.491 (0.170) | 50 | 2 (0.148) | 0.01072 |
| 21-25* | 55 | 1.618 (0.173) | 50 | 1.96 (0.162) | 0.06873 |
| 26-30*** | 55 | 1.491 (0.168) | 50 | 2 (0.140) | 0.00842 |
| All Periods*** | 330 | 1.603 (0.184) | 300 | 2.12 (0.280) | $4.76 \times 10^{-8}$ |

$^{*}$, $^{**}$, and $^{***}$ indicate a significant difference between means at the 10%, 5%, and 1% levels using a Mann-Whitney U (rank-sum) test. Standard errors are in parenthesis.




| | -9 to -8.25 | -8 to -7.25 | -7 to -6.25 | -6 to -5.25 | -5 to -4.25 | -4 to -3.25 | -3 to -2.25 | -2 to -1.25 | -1 to -0.25 | 0 | .25 to 1 | 1.25 to 2 | 2.25 to 3 | 3.25 to 4 | 4.25 to 5 | 5.25 to 6 | 6.25 to 7 | 7.25 to 8 | 8.25 to 9 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| US b=1.25 | 0 | 5.5 | 0 | 1.4 | 2.5 | 6.6 | 5 | 7.2 | 11.3 | 28.9 | 11 | 5.2 | 7.4 | 2.8 | 1.4 | 1.7 | 1.1 | 0.6 | 0.6 |
| US b=2.25 | 0 | 0 | 0 | 0.2 | 0 | 1.7 | 0.5 | 2.2 | 8.2 | 35.8 | 26.9 | 16.5 | 3.9 | 3.1 | 0.5 | 0.2 | 0.2 | 0 | 0 |
| JPN b=1.25 | 0 | 0 | 0 | 0 | 0 | 0.6 | 5.2 | 1.9 | 32.3 | 40.6 | 11.6 | 2.6 | 3.9 | 1.3 | 0 | 0 | 0 | 0 | 0 |
| JPN b=2.25 | 0 | 0 | 0 | 2.6 | 1.6 | 1.1 | 0.5 | 12.7 | 20.6 | 56.6 | 1.6 | 2.1 | 0 | 0.5 | 0 | 0 | 0 | 0 | 0 |

Figure 1.3: Difference Between the Optimal Writer Action and the Actual Writer Action Given the Contract in Japan and US Treatments

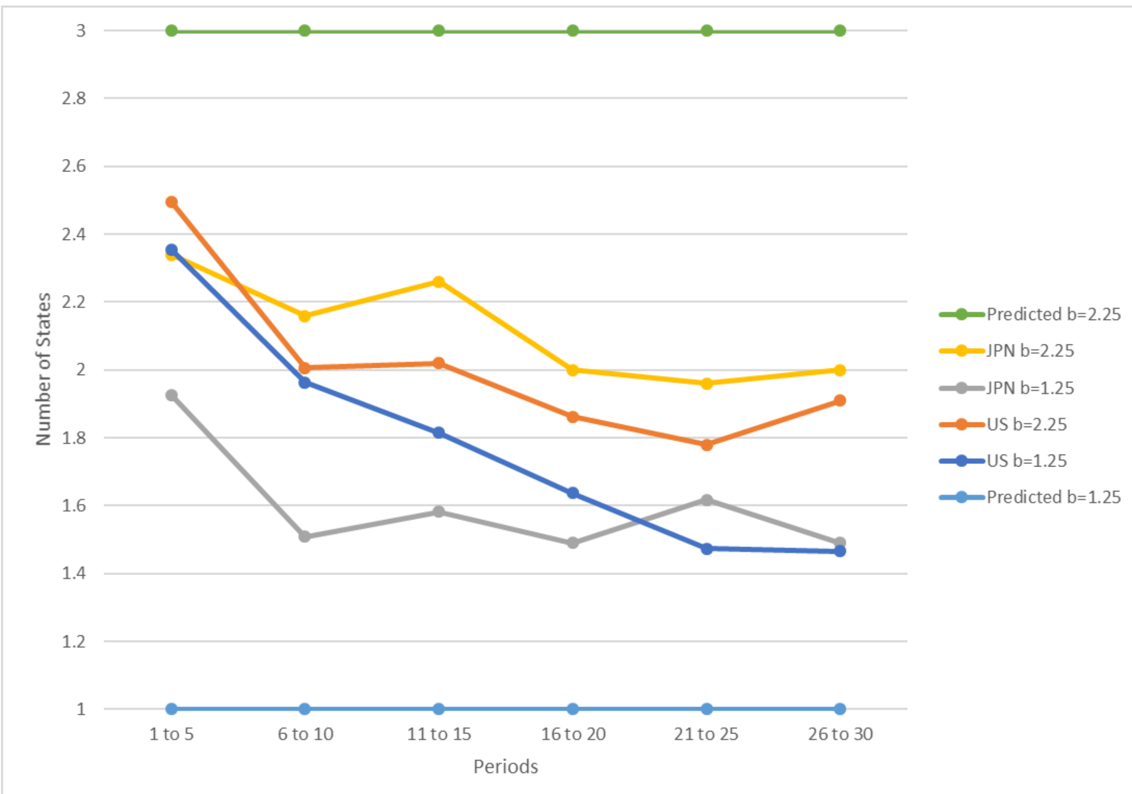



Figure 1.4: Number of States Included in Contracts in Japan and US Sessions

Table 1.12: Correlations in Each Communication Subgame

| Communication Subgame Following the Contract (Low State, High State) | b | # of Times Communication Subgame is Reached. | Predicted State-Message | Actual State-Message | Predicted Message-Action | Actual Message-Action | Predicted State-Action | Actual State-Action |
|---|---|---|---|---|---|---|---|---|
| No Contract | 1.25 | 93 | 0.776 | 0.919*** | 0.896 | 0.945 | 0.866 | 0.857 |
| No Contract | 2.25 | 23 | 0.527 | 0.572 | 0.745 | 0.629 | 0.707 | 0.641 |
| (3,7) | 1.25 | 21 | 1 | 0.198** | 1 | 0.881 | 1 | 0.161** |
| (3,7) | 2.25 | 52 | 1 | 0.554** | 1 | 0.767 | 1 | 0.251*** |

*, **, and *** indicate a significant difference from the predicted values at the 10%, 5%, and 1% levels using a t-test for differences in correlation

## 1.5 Conclusion

This paper is an initial glimpse into explaining how people write incomplete contracts when incompleteness is predicted to be optimal. The number of states included in the contract increases as bias increases, validating theoretical predictions in this paper and reinforcing messages in related papers Shavell (2006) and Heller & Spiegler (2008). Subjects wrote contracts that did not include the correct writer action given the states in the contract. Subjects in the Japanese treatment wrote contracts that were more closely aligned with predictions. It is hypothesized that this has to do with subject pool sophistication.

Additionally, this paper analyzes the relationship between the interpretation process and the writing of incomplete contracts. Some writers tended to abstain from writing contracts, and payoffs in that communication subgame were better than predicted. In general, communication subgames in which the contract specified states strictly in the middle of the state space yielded poor payoffs to subjects. Furthermore, there was a general theme of undercommunication in the communication subgames analyzed in this paper.

There are many avenues for future research that build off of this experiment. Although this paper explores how people go about writing contracts and how people go through the interpretation process presented, there is an unclear causal link between the two. It is unclear whether behavior inside of communication subgames causes different behavior in contract writing or whether behavior in contract writing drives the way players communicate. It would be revealing to analyze experiments that fix either the contract or the interpretation process to isolate how people play in absence of one of the aspects of the experiment. These kinds of explorations may also help explain why undercommunication is common in communication subgames and why communication results appear to differ from Cai & Wang (2006).

This task is difficult in similar ways to contract writing. Not only do subjects have to do calculations to determine the best action, they must also keep in mind the tradeoff between including states in a contract and not including states in a contract. This paper is one of the first attempts at exploring how subjects behave in an experiment where another party providing interpretation for gaps in the contract provides an incentive to write an incomplete contract. Hopefully, this project inspires other researchers to explore how people write incomplete contracts.

**Chapter 2**

# An Experimental Study On the Impact of Communication in Outguessing Games

Communication introduces new ways in which level-k may matter in outguessing games. Prior to playing an outguessing game, a sender sends a message to a receiver stating that the sender will play a specific action. It is predicted that the message causes players to behave according to the basic model of level-k presented in Crawford (2003): Level-0 senders are truthful and level-0 receivers believe level-0 senders. Level-k senders best respond to level-(k − 1) receivers. Level-k receivers believe that level-k senders are truthful. Subjects play five periods of the game, anonymously and randomly playing against the field in each period. This design is utilized to analyze how experience impacts a subject's level. The experiment finds that level-0 play is common in the first period of play, but vanishes almost completely by the last period, indicating that subjects do not have a stable preference for truth-telling. Play is mostly focused on levels 0, 1, and 2. The number of unidentifiable players rises over time, indicating that players play more complex strategies as they grow experienced.

**JEL**: C91

**Keywords**: *cheap-talk, level-k, experimental economics*

## 2.1 Introduction

In competitive settings, players sometimes use cheap talk as a part of their strategy. In the title match of the 2007 USA Rock Paper Scissors championship, prior to the first throw, David Borne said “let’s roll" to his opponent Jamie Langridge. The commentators noted that this statement from Borne was calling for his opponent to play Rock. Borne continued to make statements before throws during that match. Throughout the 2006 World Series of Poker Main Event, the eventual winner Jamie Gold would frequently talk to his opponents during hands. Gold would sometimes tell the complete truth about his privately held cards as a part of his strategy. In the book “Caro’s Book of Poker Tells" by Mike Caro, one section is devoted to the poker adage “weak means strong" while another section is devoted to “strong means weak." These sections emphasize that a poker player acts like they have a weak hand when they frequently hold strong cards, while players who act like they have strong hands typically hold weak cards.

When communication is feasible, Crawford (2003) proposes that players anchor on truth. This experiment tests this effect. It is predicted that with communication, subjects play according to the level-k model presented in Crawford (2003): Level-0 senders send truthful messages and level-0 receivers believe messages. Level-k senders best respond to level-(k − 1) receivers, and level-k receivers believe that a level-k sender is telling the truth about their action. Typically it is assumed by many papers in the literature that a majority of players will be level-1 or level-2 players. The experimental design allows for levels 0 through 5 to be measured, assuming that subjects are not on level 6 or above. Additionally, in each period, all senders will be randomly paired against all receivers. This means that in each period, each subject will make between five and eight choices, which provides high confidence on whether subjects play a specific level.

The goal of this paper is to show the impact of communication in outguessing games. This is accomplished in two ways. First, play with communication is compared to play without communication. This paper will compare the patterns of play to assess if communication has an impact. Secondly, level-k theory will be tested. This will provide a deeper exploration into the exact impact of pre-play communication.

Many experimental studies examine level-k models in games involving communication. Cai & Wang (2007), in their experiment testing Crawford & Sobel (1982), use level-k with an honest level 0 as an explanation for subject play. Sánchez-Pagés and Vorsatz (2007), and Holm and Kawagoe (2010) examine communication in conjunction with games that are similar to matching pennies, but in a private-information setting. Kawagoe & Takizawa (2009) provide additional evidence that supports level-k as a viable explanation of behavior in communication games. These papers examine cases of private information, whereas this project focuses on level-k in a complete information setting.

Other papers involving communication center around the idea that players are biased toward truth-telling. In this paper, this bias equates to seeing a plethora of level-0 play. Blume et al. (2001) and Cai & Wang (2007) note that subjects overcommunicate. Rode (2006) shows that subjects sometimes communicate truthfully against their benefit. Charness & Dufwenberg (2005), Gneezy (2005), Sutter (2009), and Hurkens & Kartik (2009) examine this bias in more detail and attempt to analyze why and how subjects may be truth-biased.

Two theory papers inspired this project. These papers are inspired by seminal papers by Farrell (1987, 1988), which analyze equilibria where, due to the use of pre-play communication, actions can be correctly inferred by players. Ellingsen and Östling (2010) examine all 2x2 games and use a level-k model to identify on all classes of games where communication can help or hurt coordination on Nash equilibria. Crawford (2003) uses the level-k model to explain the events of D-Day, which he models as a zero-sum game between the Axis and Allies. The primary contribution of this paper is an experiment that can specifically test whether people use level-k, with truth-telling being a level 0 and where there is no other reasonable behavioral explanation for observing that type of play. Additionally, tests can determine if level-k correlates with individual characteristics and if players' levels change over time.

This paper contributes to the growing literature on the general methodology of level-k and the measurement of level-k in a setting with communication. Arad and Rubinstein (2012) propose the 11-20 game as a test of level-k. Georganas et al. (2015)

show that level-k is not the same for the same subjects across two families of games. Heap et al. (2014) use a wide variety of framing effects to test level-k hypotheses in hide-and-seek and coordination games, which they do not find evidence to support.

Many other papers analyze games in a laboratory setting and then explain behavior using level-k. These include Stahl and Wilson (1995), Nagel (1995), Ho et al. (1998), Costa-Gomes et al. (2001), Camerer et al. (2004), Crawford and Iriberri (2007), and many others.

## 2.2 Experimental Setup and Predictions

| | | Receiver | | | | | |
|---|---|---|---|---|---|---|---|
| | | $A$ | $B$ | $C$ | $D$ | $E$ | $F$ |
| Sender | $A$ | $1,0$ | $0,1$ | $0,0$ | $0,0$ | $0,0$ | $0,0$ |
| | $B$ | $0,0$ | $1,0$ | $0,1$ | $0,0$ | $0,0$ | $0,0$ |
| | $C$ | $0,0$ | $0,0$ | $1,0$ | $0,1$ | $0,0$ | $0,0$ |
| | $D$ | $0,0$ | $0,0$ | $0,0$ | $1,0$ | $0,1$ | $0,0$ |
| | $E$ | $0,0$ | $0,0$ | $0,0$ | $0,0$ | $1,0$ | $0,1$ |
| | $F$ | $0,1$ | $0,0$ | $0,0$ | $0,0$ | $0,0$ | $1,0$ |

Table 2.1: The Basic Game Form

In this experiment, two players, a sender and a receiver, will play the simultaneous move game in Figure 2.1. The sender has a strict preference for the state matching, while the receiver has a strict preference for a specific mismatching of the state. In the communication treatment, the sender must send a message $m$ to the receiver,[1] where $m$ states "I will take action $X$[2]" for some $X \in \mathbf{A} = \{A, B, C, D, E, F\}$.

[1] The experiment uses symbols in place of letters to mitigate unconscious bias towards certain letters. In the text, using letters is more convenient and illustrative.

[2] The instructions state "I will take action $X$," while the Z-tree program states "I will select action $X$." Both statements have similar strength of meaning, so it is not expected to impact results, but it is noted here for complete disclosure.

### 2.2.1 Predictions Under No Communication

This game has a unique mixed-strategy Nash equilibrium in which each player randomizes uniformly over all pure strategies. To see that this is the only Nash equilibrium, first note that there are no pure-strategy equilibria, meaning that only mixed strategies must be considered. Additionally, due to the cyclical nature of this game, the following notation will be used: Let the actions have the cyclic order $(A, B, C, D, E, F)$. Note that, for an arbitrary action $X \in \mathbf{A}$, $X(N)$ is the action $X$ shifted $N$ actions forward in the cycle. This means, for example, that $X(6) = X$.

Suppose the sender plays a mixed strategy involving exactly $N = 2$ pure strategies. Denote those strategies $X$ and $Y$. Because the best response to a pure strategy is unique, each pure strategy has a different best response, and each best response has the same payoff, the sender makes the receiver indifferent by placing equal probability on $X$ and $Y$. Mixing uniformly between $X$ and $Y$ makes the receiver indifferent between two pure strategies $X(1)$ and $Y(1)$. However, for any receiver strategies $X(1)$ and $Y(1)$, the only way to make the sender indifferent between two best responses is to mix uniformly between $X(1)$ and $Y(1)$, which makes the sender indifferent between pure strategies $X(1)$ and $Y(1)$. If a mixed-strategy Nash equilibrium exists, both players must best respond to each other, which implies that $X(1) = Y$ and $Y(1) = X$ hold. This is a contradiction, since $X(2) = X(N) \neq X$.

This same logic applies to mixtures involving $N < 6$ pure strategies. A mixed-strategy equilibrium can only exist when $X(N) = X$, which only holds for N=6. That mixed strategy is a uniform mixture across all possible strategies.

**Prediction 1:** With no communication, subjects will play according to the unique mixed-strategy Nash equilibrium: A uniform distribution of actions will be observed.

### 2.2.2 Predictions with Pre-play Communication

With communication, the sender must send a message to the receiver. This message is a statement that the sender intends to play a specific action. After the message is sent, both players simultaneously decide on an action to take. Note that because the equilibrium of the no-communication game is unique, because communication does not change the set of payoffs, and because this is a game of complete information, communication does not change the set of Nash equilibrium outcomes.

However, communication may have an impact on players who are level-k thinkers. In particular, consider a level-k model in which a level-0 sender is assumed to always be telling the truth and a level-0 receiver is assumed to always believe the sender's message. A level-1 sender then best responds to a level-0 receiver, while a level-1 receiver believes that a level-1 sender tells the truth. This is a model used in a multitude of papers that originates from Crawford (2003). Under this level-k model, suppose that the sender plays the message "I will take action $A$." Play would then proceed according to table 2.2 depending on the level of the players playing the game. It is commonly observed in other level-k studies (Cai & Wang (2007), Georganas et al. (2015)) that the majority of the players are level-1 or level-2, with few level-0 and few level-3 players.

**Prediction 2:** With communication, suppose that message "I will take action $X$" is sent. A majority of senders will play actions $X$ (1) and $X$ (2), and a majority of receivers will play actions $X$ (2) and $X$ (3).

If it is true that subjects are level-k thinkers, subjects should see higher average payoffs than the Nash equilibrium payoff, which is $\frac{1}{6}$. In this game, subjects are rewarded for figuring out exactly what the other player is doing. It follows that without communication, subjects should not see higher average payoffs than the Nash equilibrium.

**Prediction 3:** With communication, subjects' payoffs will be higher than the Nash equilibrium payoff. Without communication, subjects' payoffs will not be higher than the Nash equilibrium payoff.

The communication level-k model is unique in that play is heavily influenced with communication. It is possible that without communication, a player playing an outguessing game can behave in a similar manner. Define the no-communication level-k model as follows: Suppose that a level-0 sender is naturally drawn to some specific action, for instance, $A$. If receivers believe that senders are drawn to action $A$, a level-0 receiver should always play action $B$. If senders then believe that receivers believe that senders are drawn to action $A$, a level-1 sender should play action $B$. This logic identically replicates the pattern of play produced with communication, illustrated in Table 2.2. Note that the no-communication level-k model is fairly robust to different initial level-0 sender beliefs: As long as players in the game believe that a level-0 sender plays $A$ more frequently than any other actions, a level-0 receiver's unique best response is $B$.

In this setting, players should not play according to the no-communication level-k model when there is no communication. With communication, a focal belief can be formed. Without communication, any level-0 assumption is arbitrary, and could only happen if there was something distinct about one specific action. It is predicted that communication is the force that causes this theory of level-k to be observed.

**Prediction 4:** Without communication, play will not be consistent with the no-communication level-k model.

Additionally, little is known about how cognitive ability correlates with level hierarchies, and about how level-k play evolves over time. Subjects' Cognative Reflection Test (CRT) score is measured. I will then test whether CRT score or experience has an influence on level of play.

**Prediction 5:** As CRT score increases, subjects will play higher levels.

**Prediction 6:** Subjects' level will increase as they play more periods of the game.

## 2.3 Experimental Design

Subjects completed the task at the Experimental Science Laboratory at the University of Arizona. The experiment was coded in z-Tree [24]. In the experiment, actions were labeled $\{\#, \%, \hat{}, +, ^*, (\}$ in place of $\{A, B, C, D, E, F\}$ respectively. Subjects participated in a total of eight sessions–four for each treatment. The no-communication treatment had a total of 54 subjects–27 senders and 27 receivers. In this treatment, senders were called "row players" and receivers were called "column players." The communication treatment also had a total of 54 subjects–27 senders and 27 receivers. Each session had between 10 and 16 people. Subjects played five periods, with roles fixed as either the sender or the receiver across all five. Within every period, each sender played the stage game with each receiver in the room. Matching was done randomly and anonymously. Subjects were paid for one play of the stage game cho-

Table 2.2: How level-k players best respond to the message "I will take action A" assuming that a level-0 Sender is truthful and that a level-0 Receiver believes a level-0 Sender

| Level | Sender Action | Receiver Action |
|---|---|---|
| Level-0 | $A$ | $B$ |
| Level-1 | $B$ | $C$ |
| Level-2 | $C$ | $D$ |
| Level-3 | $D$ | $E$ |
| Level-4 | $E$ | $F$ |
| Level-5 | $F$ | $A$ |
| Level-6 | $A$ | $B$ |
| $\vdots$ | $\vdots$ | $\vdots$ |

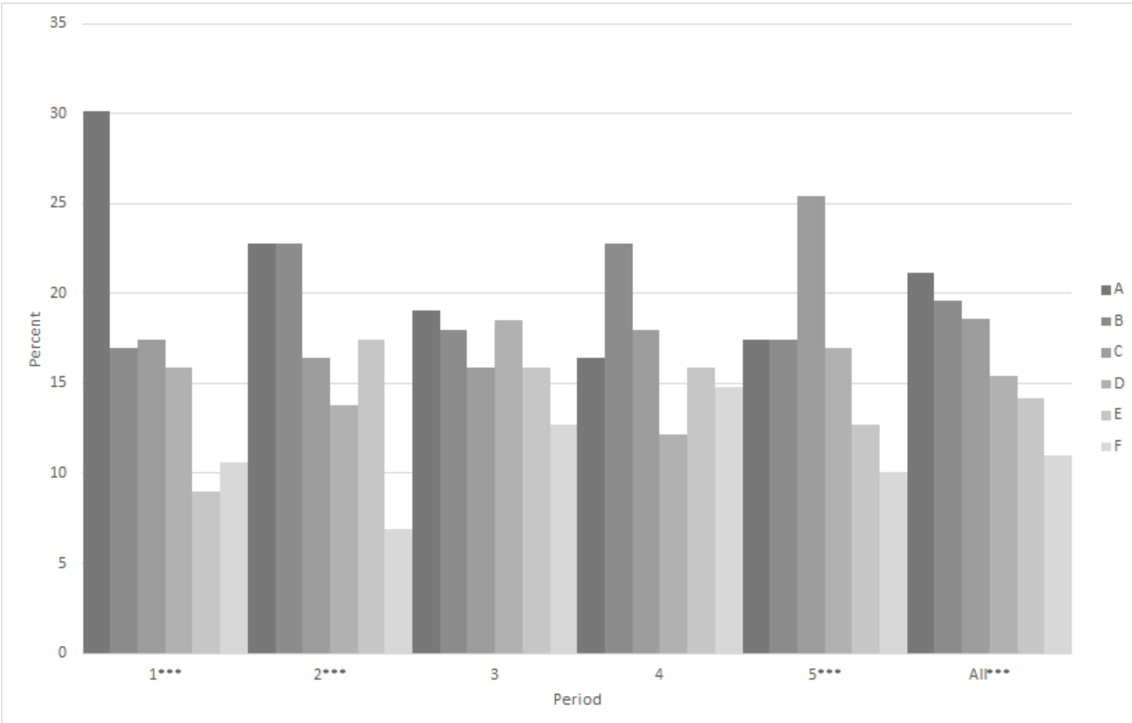
sen randomly at the end of the experiment. Each subject earned \$8 per ECU, along with a show-up fee of \$6. Additionally, in the communication treatment, subjects were asked three Cognitive Reflection Test (CRT) questions, for which they earned \$1 for each correct answer. The experiment took approximately 30 minutes for the no-communication treatment and approximately 40 minutes for the communication treatment.

## 2.4 Results

### 2.4.1 No Communication

Without communication, Figure 2.1 displays the total actions that senders took, while Figure 2.2 displays the total actions that receivers took. Also displayed are the percentage of actions in each of the five periods, as well as the percentage of actions across all periods. It is noteworthy that the distribution does not resemble a uniformly random distribution. For senders, periods three and four do not reject the null hypothesis. For receivers, periods four and five do not reject the null hypothesis. When pooling the last three periods together, senders are different from a uniform

Figure 2.1: Sender Actions No Communication



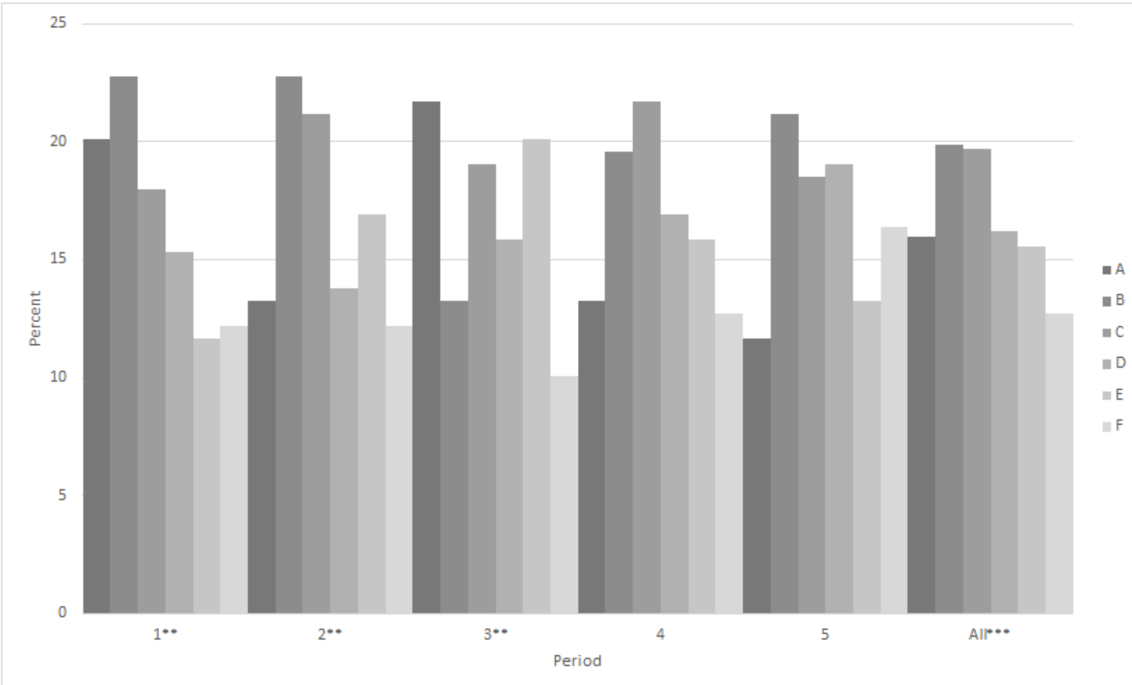

*,**, and *** indicate a significant difference to a uniform distribution of actions at the 10%, 5%, and 1% levels using a Pearson's chi-squared test.

distribution at the 5% level using a Pearson's Chi-squared ($\chi^2 = 13.33$), while the null hypothesis cannot be rejected for receivers ($\chi^2 = 8.88$).

Overall, there appears to be some adjusting and learning that points in the direction of random play, but this is not uniformly true. In particular, both senders and receivers play action $F$ (called '(' in the experiment) less frequently than other actions. This then leads to a follow up question: Does this biasing of actions impact observed play in experiments?

In this game, if a player has a belief that their opponent plays an action a higher percentage of the time than any other action, the best response is a pure strategy. If the distribution of actions has an impact on the way subjects choose actions in

Figure 2.2: Receiver Actions No Communication




*, **, and *** indicate a significant difference to a uniform distribution of actions at the 10%, 5%, and 1% levels using a Pearson's chi-squared test.

the no-message treatment, subjects should play a pure strategy in response. The distributions of senders and receivers who take the same action over 50% of the time in each period are displayed in Tables 2.6 and 2.7. In any given period, over half of subjects are not playing the same action more than 50% of the time. Additionally, very few subjects, if any, seem to be taking advantage of this bias. If senders note that receivers are biased toward picking B, senders should pick B more often in subsequent periods, which is not observed. If receivers know that senders are biased toward picking A, receivers should pick B more, which is also not observed. Thus, the distribution not resembling the Nash equilibrium appears to be caused by unconscious bias.

**Result 1:** Prediction 1 is not supported. Subjects do not randomize over all actions uniformly without communication. This is attributed to an unconscious bias away from action $F$.

Table 2.3: Communication Sender Levels Using > 50% Threshold

| | Period | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 |
| 0 | 10/27 | 4/27 | 5/27 | 4/27 | 3/27 |
| 1 | 8/27 | 13/27 | 6/27 | 8/27 | 4/27 |
| 2 | 3/27 | 3/27 | 7/27 | 6/27 | 6/27 |
| 3 | 0/27 | 0/27 | 0/27 | 1/27 | 1/27 |
| 4 | 1/27 | 0/27 | 0/27 | 0/27 | 0/27 |
| 5 | 0/27 | 4/27 | 2/27 | 1/27 | 2/27 |
| No Level | 5/27 | 3/27 | 7/27 | 7/27 | 11/27 |

### 2.4.2 Communication

With communication it is predicted that play proceeds according to Crawford (2003): level-0 senders match the action with the message, level-k receivers believe level-k senders and respond accordingly, and level-k senders best respond to level-(k − 1) receivers. The appendix displays the distribution of messages, the overall distribution of actions, and the overall distribution of levels for senders and receivers.

To accurately interpret overall play, level-k is measured at an individual level using a 50% threshold: If, in a given period, over half of a player's actions are consistent with a level of play, that player is classified as that level. This threshold has a high degree of accountability in this game, since for any message there is only a single action that is consistent with a specific level.

The number of subjects classified in each level in each period of play is displayed in Tables 2.3 and 2.4. In the first period, over half of receivers and over a third of senders are consistent with level-0 play. Additionally, many senders are consistent with level-1 play. In the second period, level-0 play diminishes greatly for both senders and receivers. In period two, almost half of senders play consistently with

Table 2.4: Communication Receiver Levels Using > 50% Threshold

| | Period | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 |
| 0 | 16/27 | 10/27 | 6/27 | 5/27 | 5/27 |
| 1 | 6/27 | 11/27 | 12/27 | 10/27 | 8/27 |
| 2 | 0/27 | 0/27 | 0/27 | 3/27 | 3/27 |
| 3 | 1/27 | 0/27 | 0/27 | 0/27 | 0/27 |
| 4 | 1/27 | 1/27 | 1/27 | 1/27 | 0/27 |
| 5 | 1/27 | 1/27 | 1/27 | 1/27 | 0/27 |
| No Level | 2/27 | 4/27 | 7/27 | 7/27 | 11/27 |

Table 2.5: Among No Level Subjects, How Many Subjects Randomize

| | Period | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 |
| Sender | 1/5 | 2/3 | 5/7 | 6/7 | 7/11 |
| Receiver | 1/2 | 3/4 | 5/7 | 3/7 | 4/11 |

level 1, while receivers mostly play consistently with level 0 and level 1. By period five, over a third of senders and receivers do not play consistently with a specific level. Additionally, subjects are spread out between levels 0, 1, and 2.

One main takeaway from the data is that, surprisingly, level-0 prevails in the first period of play but then diminishes significantly as subjects gain experience. This strongly indicates that subjects are initially biased to be honest. This bias changes with experience, however, and level-k appears to explain more (but not all) of the data as subjects play more. Additionally, receivers are primarily level-0 and level-1, while senders are more split between levels 0, 1, and 2. This could be due to differences between how senders take actions and how receivers take actions.

Another surprising result is the emergence of play that is not classifiable as any level. As subjects become more experienced, subjects tend to differentiate their play more. It begs the question of whether some subjects are becoming sophisticated over time by playing truly randomly or whether subjects are varying the level they are playing on within the same period. To test this, Table 2.5 lists the number of subjects in each period that are "randomizing." In any given period, subjects whose play is consistent with at most three consecutive levels are considered to be not random, while all other subjects are considered to be random. For example, this means that in the last period, 7 out of 11 receivers only played actions consistent with three consecutive levels, while 4 out of 11 receivers did not play actions that were consistent with at most three consecutive levels. Analyzing this data, it is evident that there is a split between subjects who play mixtures of levels and subjects who play randomly.

One notable oddity in the data is the presence of players who are on level 5. While there are not many of these players, it is worth discussing why such players may exist. A player who is altruistic toward honest players would be incentivized to play level 5. If a portion of players want someone to get a payoff but also want to reward honesty, this explains why a fraction of the subject pool would continue to play an action that is highly unlikely to get them a payment.

Overall, neither prediction fully explains the data. In the first period, there is a large bias toward honesty, but the remainder of the subjects are mostly classified on one level or another. As subjects play more, the honesty bias largely disappears, but many subjects start either to randomize or to mix between levels. This suggests that the way in which communication influences thinking is more complex than the literature gives it credit for. Overall, players do anchor their play on senders potentially sending honest messages. However, the distribution of levels in the last period of the game is far different than the literature would predict.

**Result 2:** Predction 2 is not supported. In the first period, many subjects' actions are influenced by a bias for truth-telling. This bias goes away over time. Level-k explains a majority of the data across all periods, but the distribution of levels is different than the literature suggests.

Table 2.6: Senders Who Play Same Action Using > 50% Threshold No Communication

| | Period | | | | |
|---|---|---|---|---|---|
| Action | 1 | 2 | 3 | 4 | 5 |
| A | 5/27 | 4/27 | 2/27 | 1/27 | 2/27 |
| B | 3/27 | 4/27 | 2/27 | 3/27 | 2/27 |
| C | 1/27 | 1/27 | 1/27 | 1/27 | 4/27 |
| D | 1/27 | 1/27 | 1/27 | 0/27 | 1/27 |
| E | 0/27 | 2/27 | 0/27 | 1/27 | 1/27 |
| F | 1/27 | 0/27 | 1/27 | 1/27 | 0/27 |
| Mixture | 15/27 | 20/27 | 20/27 | 19/27 | 15/27 |

Table 2.7: Receivers Who Play Same Action Using > 50% Threshold No Communication

| | Period | | | | |
|---|---|---|---|---|---|
| Action | 1 | 2 | 3 | 4 | 5 |
| A | 3/27 | 0/27 | 3/27 | 1/27 | 0/27 |
| B | 2/27 | 3/27 | 0/27 | 2/27 | 2/27 |
| C | 1/27 | 1/27 | 1/27 | 2/27 | 1/27 |
| D | 0/27 | 0/27 | 0/27 | 0/27 | 1/27 |
| E | 0/27 | 1/27 | 1/27 | 1/27 | 1/27 |
| F | 0/27 | 1/27 | 0/27 | 0/27 | 1/27 |
| Mixture | 21/27 | 20/27 | 20/27 | 20/27 | 19/27 |

Table 2.8: Average Earnings

| | | Period | | | | | |
|---|---|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 | 5 | Overall |
| Communication | Sender | 0.294*** (n = 187, .389) | 0.262*** (n = 187, .441) | 0.278*** (n = 187, .449) | 0.246*** (n = 187, .432) | 0.246*** (n = 187, .432) | 0.265*** (n = 935, .441) |
| | Receiver | 0.267*** (n = 187, .444) | 0.337*** (n = 187, .474) | 0.224** (n = 187, .419) | 0.257*** (n = 187, .438) | 0.193 (n = 187, .395) | 0.256*** (n = 935, .436) |
| No Communication | Sender | 0.185 (n = 189, .389) | 0.196 (n = 189, .398) | 0.148 (n = 189, .356) | 0.180 (n = 189, .385) | 0.196 (n = 189, .398) | 0.181 (n = 945, .385) |
| | Receiver | 0.148 (n = 189, .356) | 0.180 (n = 189, .385) | 0.148 (n = 189, .356) | 0.153 (n = 189, .361) | 0.138 (n = 189, .345) | 0.153 (n = 945, .360) |

*,**, and *** indicate a significant difference from the Nash equilibrium payoff of 0.166 at the 10%, 5%, and 1% levels using a one-tailed t-test. Sample size and standard errors are in parenthesis.

Looking at Table 2.8, it is clear that subjects do better with communication than without communication. With communication, in all periods for senders and in all but the last period for receivers, the payoff is statistically different from the Nash equilibrium payoff at the 1% level using a one-tailed t-test. Without communication, in all periods for all roles payoffs are not statistically different from the Nash equilibrium payoff using a one-tailed t-test. This indicates that communication is allowing players to coordinate their actions more frequently than in the absence of communication.

**Result 3:** Prediction 3 is supported. Almost all individual periods of play earn significantly more with communication than the Nash equilibrium, while without communication subjects never earn significantly more than the Nash equilibrium.

To see that play without communication cannot be explained by the no-communication level-k model, displayed in Tables 2.6 and 2.7 are the number of subjects that play the same action more than 50% of the time in a given period. If play proceeded according to the no-communication level-k model, subjects should always play the same action. Allowing for some error, a majority of the subjects do not play the same action in each period. This means that play cannot be explained by this particular model of level-k.

**Result 4:** Prediction 4 is supported. The distribution of actions without communication cannot be explained by the no-communication level-k model. This implies that communication fundamentally changes the way that subjects take actions.

To analyze the impact of CRT scores and experience, I utilize the following linear regression, separated for senders and receivers:

$$Level_{it} = \alpha + CRT_i + Period2 + Period3 + Period4 + Period5 + \epsilon_{it}$$

where $Level_{it} \in \{0, 1, 2, 3\}$ is the level of player $i$ in period $t$, $CRT$ is a dummy variable that is 1 if the CRT score of individual $i$ is 2 or 3 and is 0 if the CRT score of individual $i$ is 0 or 1, and Period$t$ is the $t$th period in the game. Because of the presence of players with no level, and because play above level-3 is largely absent, the sample is restricted to periods in which play was on levels 0 through 3. This

Table 2.9: OLS Regression of Period and CRT on Level Using Level 0 to Level 3 Only

| Variable | Subject Type | |
|---|---|---|
| | Sender | Receiver |
| Intercept | 0.232 (0.194) | 0.121 (0.137) |
| CRT | $0.652^{***}$ (0.167) | $0.518^{***}$ (0.123) |
| Period2 | 0.262 (0.228) | 0.107 (0.175) |
| Period3 | $0.408^{*}$ (0.234) | 0.229 (0.183) |
| Period4 | $0.498^{**}$ (0.231) | $0.394^{**}$ (0.184) |
| Period5 | $0.659^{**}$ (0.252) | $0.398^{**}$ (0.190) |
| Degrees of Freedom | 86 | 90 |
| Adjusted $R^2$ | 0.179 | 0.2 |

$^{*}$,$^{**}$, and $^{***}$ indicate p values that are significant at the 10%, 5%, and 1% levels. Standard errors are in parenthesis.

means that the data set resembles an unbalanced panel. Note that individual fixed effects are not accounted for in this model, as they are highly correlated with the CRT score.

The results of this regression are in Table 2.9. The CRT variable is significant at the 1% level for both senders and receivers. Additionally, periods 4 and 5 are significant at the 5% level for senders and receivers, while period 3 is significant at the 10% level for senders. Overall, the regression results indicate that later periods have an impact on the levels that subjects choose, and that CRT is heavily linked to the level of play.

**Result 5:** Predictions 5 and 6 are supported. Both CRT and time have a strongly positive impact on the selected level of play when restricting the sample to levels 0-3.

### 2.4.3 Discussion of Increase in No-Level Play

One observation about the data that is surprising is that over time there appears to be an increase in play that is not classified as any particular level. To test the impact of experience on whether a subject is classified as no level, the following regression is utilized:

Table 2.10: OLS Regression of Period and CRT on No Level

| Variable | Subject Type | |
|---|---|---|
| | Sender | Receiver |
| Intercept | $0.274^{***}$ (0.096) | 0.135 (0.090) |
| CRT | $-0.133^{*}$ (0.077) | $-0.10341$ (0.072) |
| Period2 | $-0.074$ (0.115) | 0.074 (0.112) |
| Period3 | 0.074 (0.115) | 0.185 (0.112) |
| Period4 | 0.074 (0.115) | 0.185 (0.112) |
| Period5 | $0.222^{*}$ (0.115) | $0.333^{***}$ (0.112) |
| Degrees of Freedom | 129 | 129 |
| Adjusted $R^2$ | 0.038 | 0.052 |

$^{*}$,$^{**}$, and $^{***}$ indicate p values that are significant at the 10%, 5%, and 1% levels. Standard errors are in parenthesis.

$$NoLevel_{it} = \alpha + CRT_i + Period2 + Period3 + Period4 + Period5 + \epsilon_{it}$$

where $NoLevel_{it}$ is a dummy variable that equals 1 when a subject is classified as 'No Level', $CRT$ is a dummy variable that is 1 if the CRT score of individual $i$ is 2 or 3 and is 0 if the CRT score of individual $i$ is 0 or 1, and Period$t$ is the $t$th period in the game. The results of the regression are displayed in Table 2.10. The last period of play has a significant impact for both players on whether a subject is a level or not, while CRT has a small significant negative impact for the Sender and no significant impact for the receiver. Although time does seem to impact whether subjects are classifiable as some level, it is worth noting that the adjusted $R^2$ is incredibly small.

## 2.5 Concluding Remarks

The analysis in this paper shows that in outguessing games, communication fundamentally impacts how players make decisions. Additionally, it utilizes a unique experimental approach to allow for an accurate measurement of level-k and to study the effects of experience. Overall, it is clear that level-k explains some of the behavior in this experiment, but not all of it. Additionally, it is shown that another independent measure of cognitive ability, the CRT score, is strongly linked to higher levels of play.

The results of this paper demonstrate two key issues regarding level-k. Firstly, play tends to be very honest and the beginning of the experiment, and that honesty disappears quickly as subjects gain experience. This indicates that for initial plays of a game with communication, Gneezy (2005) may be correct in assessing that senders may initially feel guilty about lying. It also seems to be the case, as is demonstrated in other communication experiments, that receivers are more heavily truth biased. This bias disappears more slowly for the receivers than for the senders. It is mildly surprising that players behave this way in a game with no mildly equitable outcome, which indicates that this is a strong bias that should be taken seriously across all communication games.

Secondly, play changes over time. Players play more complex strategies in the sense that play is less likely to fall into the bucket of a specific level over time. Because there are six actions, a 50% threshold on being a specific level is a strong indicator of a level, so it is fascinating that players diversify their level of play more over time. This is counter to the intuition level-k provides, as that theory would predict cycles of play where levels continuously increase.

**Chapter 3**

# Interpretation Rules for Incomplete Contracts: A Laboratory Experiment

This paper provides an experimental test for incomplete contracting theory where interpretation is crucial. The paper tests simplified versions of models detailed by Heller & Spiegler (2008) and Shavell (2006). In the experiment, one player takes the role of a Writer of a contract, while the other player takes the role of a Decider who decides on a rule of interpretation to be used. Interpretation is used in the cases where the contract does not specify an action for a state of the world. The experiment uses a 2x2 experimental design, where the order of play is changed in one dimension and there is an increasing conflict of interest between the two players in the other dimension. As the conflict of interest grows, contracts should become more obligationally complete. The experimental results support that prediction. With few exceptions, play is found to be in accordance with subgame perfect equilibrium.

**JEL**: C90, D23, D86, K12

**Keywords**: *incomplete contracting, interpretation in contracts, experimental economics*

## 3.1 Introduction

Interpretation plays a central role in how contracts are written. In construction, for example, it is too costly to write a fully detailed contract. These contracts rely on interpretation when the present state is not mentioned in the contract. This interpretation is carried out either by the person whose responsibility it is to take an action or by a court. Although the kind and degree of incompleteness in a contract may be identifiable (Mansor, Rashid [2014]), it is impossible to map data on incomplete contracts directly to theory. The goal of this paper is to examine whether the way contracts are interpreted and written matches existing theory models in a laboratory experiment.

This experiment will examine a setting similar to Shavell (2006) and Heller & Spiegler (2008). These two papers focus on how people write incomplete contracts and how interpreting parties optimally interpret contracts. There are two agents: a Writer and a Decider. The Writer writes a contract that may be incomplete, because writing a contract that specifies some action for every possible state is too costly due to the plethora of possible states. The Decider provides an interpretation in the case that the state falls into a gap. Following the Writer's contract and the Decider's interpretation, the state is drawn. An action for this state is either written into the contract or is provided by the interpretation of the Decider.

In Shavell and in Heller & Spiegler (H&S), the Writer writes a contract that specifies a number of terms and is either restricted by a complexity bound (H&S) or is subject to a cost per term (Shavell). Each term in the contract dictates a specific action to be taken when one of the states listed in the term is realized. There are different orders of play in the two papers. In Shavell, the Decider publishes the interpretive rule first, while in H&S, the Writer writes a contract first. Both cases are relevant. In some instances, an interpretive rule may be published by a court, in which case the rules of interpretation are common knowledge when someone goes to write a contract with a gap in it. In other instances, when a contract written has no standard interpretation, an interpretive clause may not be defined, in which case the party interpreting the contract must decide on an interpretation after the contract is written. In addition, increasing conflict between the party interpreting a contract and the party writing the contract is predicted to affect how complete a contract will be. Both papers illustrate a key problem: When the Writer writes a contract, including an extra state gives the Writer more control over the action taken and gives the Decider less control over the action taken. This problem of how to optimally assign control is the main focus of this paper.

This experiment will test four primary predictions. A main result of H&S is that contracts become more obligationally complete as the conflict of interest grows. Another main result is that each contract should include either the lowest state or the highest state. In addition to testing these predictions of equilibria, this experiment will test to see if the actions in contracts are optimal given the states included in the contract. This experiment will also test for whether Decider actions are subgame perfect. The experiment includes four treatments in total, using a 2x2 design. Treatments vary over the levels of conflict of interest and over the order of play. The conflict treatment has a low level of conflict and a high level of conflict. The variation in the order of play is so that both Shavell and H&S are represented.

Over the last ten rounds of play, the contracts are more complete in the high-conflict of interest treatment, providing support for the predictions. Participants also tend to place contracts such that one of the extremes of the state space is covered in the contract. In addition, there is evidence that subjects write actions into their contract that are optimal given the states included. Writers who move first write contracts that more consistent with equilibrium predictions than a writer who plays randomly. Deciders mirror this prediction when the conflict is high, but when the conflict is low Deciders overwhelmingly pick the action in the middle of the state space.

This observation, along with the post-experimental questionnaire, indicate that fairness and reciprocity could play a role in the distribution of Default Actions and Writer Actions. In employment contracts, fairness and reciprocity are sometimes implied by the terms of the contract. This could occur both on the employer's side, when employers are expected to give employees bonuses for good work, and on the employee's side, when employees are expected to work overtime.

Exploring this more, a level-k model is analyzed in this setting. When a level-k model in which level 0s randomize uniformly across all actions that are not dominated is tested, a level-k test fails, as too many subjects are classified as level-0. This could be due to the complexity of the game or to a bad starting point for level-0.

Dye (1985) details a model in which contracts are costly to write. Because of the cost, firms can decide to write a contract that does not cover all states of the world. This idea that contractual incompleteness can be caused by a writing cost is echoed in Shavell (2006) and Heller & Spiegler (2008), who focus on intentionally incomplete contracts in settings with interpretation rules. There are many important papers discussing incomplete contracts following Dye, including Simon (1951), who discusses incompleteness in labor contracts.

There is also a modest experimental literature on incomplete contracting. However, the literature tends to focus on the hold-up problem and on how people invest, whereas this experiment focuses on how contracts are written in settings where people provide interpretive rules for contracts. Hackett (1993) is a primary example of this. Fehr, Powell, & Wilkening (2014) aim to test the performance of Maskin & Tirole's subgame-perfect implementation mechanism in a setting with observable but non-verifiable effort, and they find that this mechanism fails to achieve good outcomes in an experimental setting.

Within the hold-up literature, many papers analyze various behavioral influences. Dufwenberg, Smith, & Van Essen (2013) analyze negative reciprocity in conjunction with the hold-up problem. Ellingsen & Johannesson (2004) look at promises and threats in this context.

In addition to this literature on the hold-up problem, there are many recent papers that analyze the best way for contracts to be written in different settings. Fehr et al. (2007) has a paper that looks at fairness in contracting, analyzing bonus contracts. Brandts, Ellman, & Charness (2015) analyze how communication plays a role in rigid and flexible contracting.

Cai & Wang (2006) analyze communication games in an experimental setting using a level-k model. Unlike this paper, they find that level-k supports subjects' play in a communication game.

Section two of this paper will outline the model and the predictions. Section three will discuss the experimental design and the results. Section four will conclude the paper.

## 3.2 Theoretical Predictions

### 3.2.1 Description

There are two players: A Writer and a Decider. The Writer has payoff $\pi_W = 20- \mid 2s - a \mid$, and the Decider has payoff $\pi_D = 20- \mid 2s - a + b \mid$, where $s \in S = \{1, 2, 3, 4, 5, 6\}$ is a state of the world drawn from a uniform distribution over the state space $S$, $a \in \mathbb{N}$ is the action taken, and $b > 0$ is the bias term.

To determine the action that is taken, the Writer writes a contract (or a rule) that is a triplet $c = (s_{low}, s_{high}, a_W)$, where $s_{low} \in S$, $s_{high} \in S$, $a_W \in \mathbb{N}$, and $s_{low} \leq s_{high}$. The 'event' is denoted $e = [s_{low},\ s_{high}]$. $a_W$, will sometimes be referred to as the 'rule action,' since this is the action named in the rule that the Writer writes. In the case that the realized state $s \in e$, $a = a_W$. In simple language, a contract has one term in it that states: If the state falls between $s_{low}$ and $s_{high}$, take action $a_W$.[1]

A few terms that will be used throughout the paper:

> A contract in this setting will be considered **obligationally complete** if $s_{low} = 1$ and $s_{high} = 6$, and will be considered incomplete otherwise.
>
> A contract is said to have a **gap** if the contract is incomplete. The **gap** is the set of states not covered by the contract $\{s \notin e\} = \{s < s_{low} \cup \ s > s_{high}\}$.
>
> A **lacuna** in a contract is either $\{s < s_{low}\}$ or $\{s > s_{high}\}$. The union of all **lacunae** is the gap. In this setting, a contract contains at most 2 **lacunae**.

For example, suppose that the the Writer writes the contract $c = (s_{low}, s_{high}, a_W) = (2, 4, 7)$. The gap in the contract is the set $\{1, 5, 6\}$. There are two lacunae, which are the sets $\{1\}$ and $\{5, 6\}$.

The Decider publishes a default action $a_D$. In the case that $s < s_{low}$ or $s > s_{high}$, $a = a_D$. That is to say, when the state falls into a gap, the default action takes place. When the state is between the low state and the high state, the action specified in the contract will be taken instead.

[1] This experiment will allow only one term in the contract, and all of the results and predictions are based on this. However, Shavell and Heller & Spiegler allow for a much richer space of contracts.

Continuing the example above, suppose that the Decider decides $a_D = 12$. If $s \in \{2, 3, 4\}$ , then $a = a_W = 7$. If $s \in \{1, 5, 6\}$ , then $a = a_D = 12$.

The experiment utilizes two versions of this model to reflect the difference between Heller & Spiegler (2006) and Shavell (2008):

> (Wfirst) The Writer moves first, publishing a contract $c = (s_{low}, s_{high}, a_W)$. The Decider observes $c$ and then chooses $a_D$. The state is drawn after both players have made their decisions. (H&S)
>
> (Dfirst) The Decider moves first, publishing a default action $a_D$. The Writer observes $a_D$ and then writes a contract $c = (s_{low}, s_{high}, a_W)$ . The state is drawn after both players have made their decisions. (Shavell)

The remainder of the paper will use Wfirst and Dfirst when referring to the two different versions of the game.

Here, a small discussion is warranted about the differences between the models in this paper, in H&S, and in Shavell. The model presented in Shavell (2006) is a generalized version of Dfirst: payoffs for each player are not specified, and instead of just interpreting a gap, the Decider is allowed to interpret terms in a contract as well, which is not allowed here. Wfirst is a discretized version of H&S, with a couple of other minor differences. Here, the payoffs are linear in the difference between the action and the state, whereas in H&S, the payoffs are quadratic loss functions. In addition, H&S allow for a more complex set of rules, where either multiple intervals can map to an action or where the allowed contract can have multiple terms. In addition, this experiment multiplies the state by two in the payoff function to get rid of the need for non-integer actions.

The next section will analyze the models of Wfirst and Dfirst using a subgame-perfect equilibrium. It will cite the result from the appropriate paper and, if necessary, will prove the analog of the result.

### 3.2.2 Observations

**Observation 6.** *The optimal action that the Writer chooses is such that* $2\mathbf{E}[s \mid s \in e] - a_W = 0$. *If* $2\mathbf{E}[s \mid s \in e]$ *is odd,* $a_W = 2E[s \mid s \in e] \pm 1$ *is also optimal. then In Wfirst,* *The optimal default action the Decider chooses is such that* $2E[(s \mid s \notin e)] - a_D = -b$. *If* $2E[(s \mid s \notin e)]$ *is odd,* $a_D = 2E[(s \mid s \notin e)] + b \pm 1$ *is also optimal.*

This observation trivially arises from how the payoffs are setup, and this experiment uses it to test whether Writers are expected payoff maximizers. Multiple actions being optimal is a result of a lack of risk aversion in the model.

**Observation 7.** *All SPE are solved for by doing backward induction: For any decision that the first mover could do, the optimal action for the second mover is found. Given any particular combination of actions that yields a Nash equilibrium within each proper subgame, the first mover picks an action that maximizes their payoff. All possible SPE paths are detailed in Table 3.1.*

This observation is obtained through writing the game tree in detail for each treatment. There are a few noteworthy things about the set of subgame perfect equilibria. In Wfirst, as $b$ increases from 2 to 10 the number of states included in a contract in an SPE weakly increases. The degree of incompleteness in Wfirst is specified completely in H&S under quadratic payoffs, and in their setting it is generally true that increasing $b$ increases the size of the optimal contract. Note that in Dfirst there is not a (weak) increase in the size of contracts due to the presence of equilibria in which the Decider dedicates to a low action that favors the Writer, causing the Writer to best respond by writing a contract that covers few states.

It is also noteworthy that in each almost every SPE a gap is also a lacuna. With a small degree of risk aversion, as is the case in H&S, this is easy to show. In this setting, in Wfirst b=10 a contract with $s_{low} = 2$ and $s_{high} = 5$ can be supported as a SPE due to the fact that all default actions yield the same payoff.

### 3.2.3 Predictions

1 Each gap, if it exists, will be a lacuna. That is, $s_{low} = 1$ or $s_{high} = 6$.

A majority of equilibria have the feature that a gap is a lacuna. This has an intuition that both Writers and Deciders receive bad payoffs when the default action covers two different lacunae, and both receive good payoffs when the default action covers a single lacuna. The exception to this intuition in Observation 7 came from

| $(s_{low},\ s_{high},\ a_W), a_D$ | Dfirst | Wfirst |
|---|---|---|
| $b=2$ | $(5,6,12),6$ | $(1,2,2),10$ |
| | $(5,6,12),7$ | $(1,2,3),10$ |
| | $(1,3,4),11$ | $(1,2,4),10$ |
| | $(1,3,4),12$ | $(5,6,10),6$ |
| | $(4,6,10),5$ | $(5,6,11),6$ |
| | $(4,6,10),6$ | $(5,6,12),6$ |
| | $(1,4,6),12$ | $(1,3,4),12$ |
| | $(1,4,6),13$ | $(4,6,10),6$ |
| | $(1,4,6),14$ | $(1,4,4),12$ |
| | | $(1,4,5),12$ |
| | | $(1,4,6),12$ |
| | | $(3,6,8),4$ |
| | | $(3,6,9),4$ |
| | | $(3,6,10),4$ |
| $b=10$ | $(5,6,10),7$ | $(2,5,6),12$ |
| | $(5,6,11),7$ | $(2,5,7),12$ |
| | $(5,6,12),7$ | $(2,5,8),12$ |
| | $(1,3,4),12$ | $(1,6,6)$, Anything |
| | $(1,4,5),14$ | $(1,6,7)$, Anything |
| | $(1,4,6),12$ | $(1,6,8)$, Anything |
| | $(1,4,6),13$ | |
| | $(1,4,6),14$ | |
| | $(1,5,6),17$ | |
| | $(1,5,6),18$ | |
| | $(1,6,8),18$ | |
| | $(1,6,8),19$ | |
| | $(1,6,8),20$ | |
| | $(1,6,8),21$ | |
| | $(1,6,8),22$ | |

Table 3.1: SPE Paths

a corner case where the written contract is such that every default action yields the same payoff.

2 The action the Writer writes in the contract maximizes the Writer's payoff given the contract structure.

The contract that the Writer writes is such that $a_w = s_{low} + s_{high}$. If $s_{low} + s_{high}$ is odd, then it is also optimal that $a_w = s_{low} + s_{high} \pm 1$.

3 As $b$ increases from 2 to 10, the number of states covered in a contract increases under both Wfirst and Dfirst.

For Wfirst, this prediction comes straight from Observation 7. For Dfirst, this prediction is supported by the intuition that as the conflict of interest grows between the Writer and the Decider, the Writer wants to control more of the state space. Conditional on the Decider picking a specific default action, the Writer wants to include the same number of states. However, if the Decider picks higher default actions, which they may with a higher bias, the Writer will respond by writing contracts that give the Decider less control.

4 The first mover in each treatment takes an action consistent with the one of the non-dominated SPE classified in table 3.1.

Prediction 4 relies on the first mover being able to do some degree of backward induction. Prior literature shows that subjects fail at higher degrees of backwards induction (Binmore et al. [2002]). However, as there are many equilibria, this prediction tests whether first movers select into specific equilibria if they are playing in equilibrium.

## 3.3 Experimental Design

For this experiment, Subjects played the game in Z-Tree (Fischbacher [2007]) at the Experimental Science Laboratory at the University of Arizona. There were 8 total sessions (2 per treatment), with 8-12 subjects in each session, all of whom were

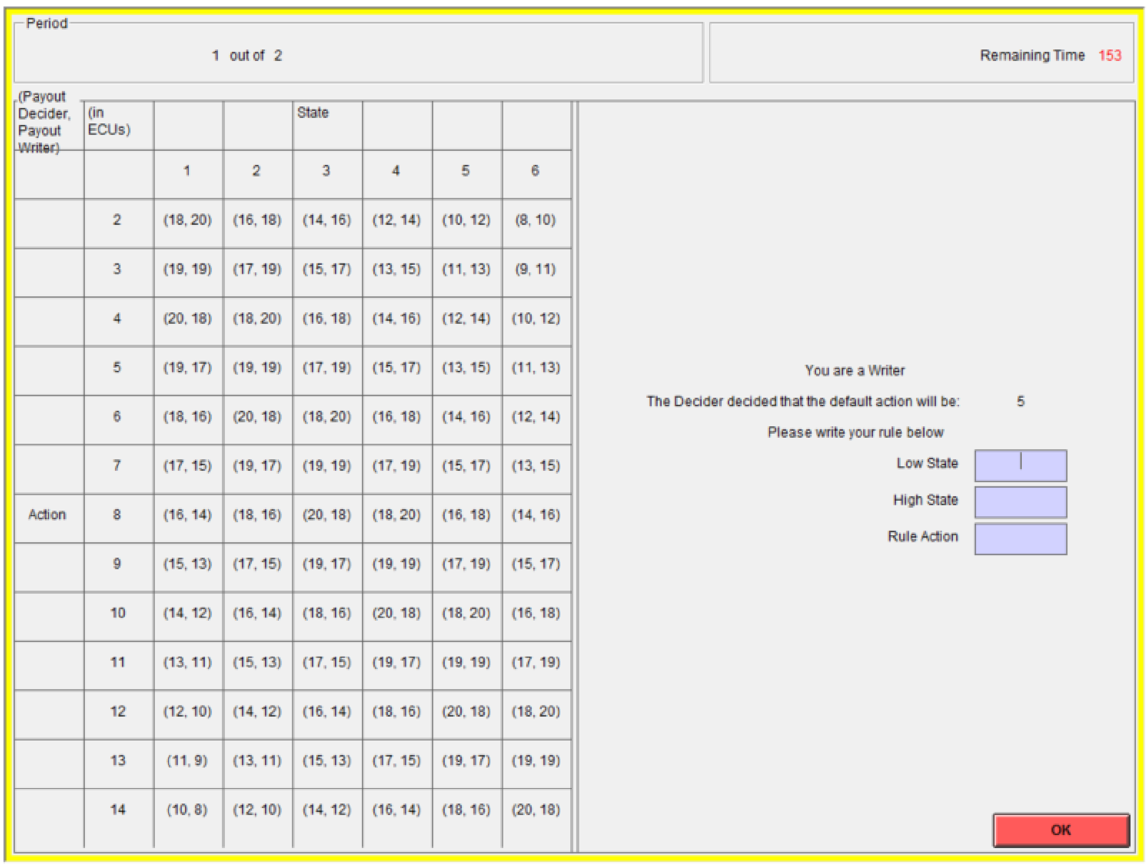
University of Arizona students. Each subject played 2 practice rounds of the game with themselves, and then were quizzed about the results of the practice rounds. Following this, each person was randomly assigned to be either a Writer or a Decider and then played 15 rounds of the game, being anonymously and randomly matched with a partner in each round. Subjects were paid for 2 of the 15 rounds of play, chosen randomly by them, and in addition were paid a $6 show-up fee. The only change between the theory covered above and the experiment is that the experiment restricted the action space to be integers that are undominated for both players. So in the $b = 2$ treatments, the actions possible were the integers between 2 and 14, while in the $b = 10$ treatments, the actions possible were the integers between 2 and 22.

Before the experiment began, the instructions were read aloud and the subjects were allowed to look over the instructions for five minutes. At the end of each practice round, there was a quiz on what action was taken based on the state drawn and what payoffs each player would have received. The subjects were paid in Experimental Currency Units, and received 30 cents for each ECU they earned.

## 3.4 Experimental Results

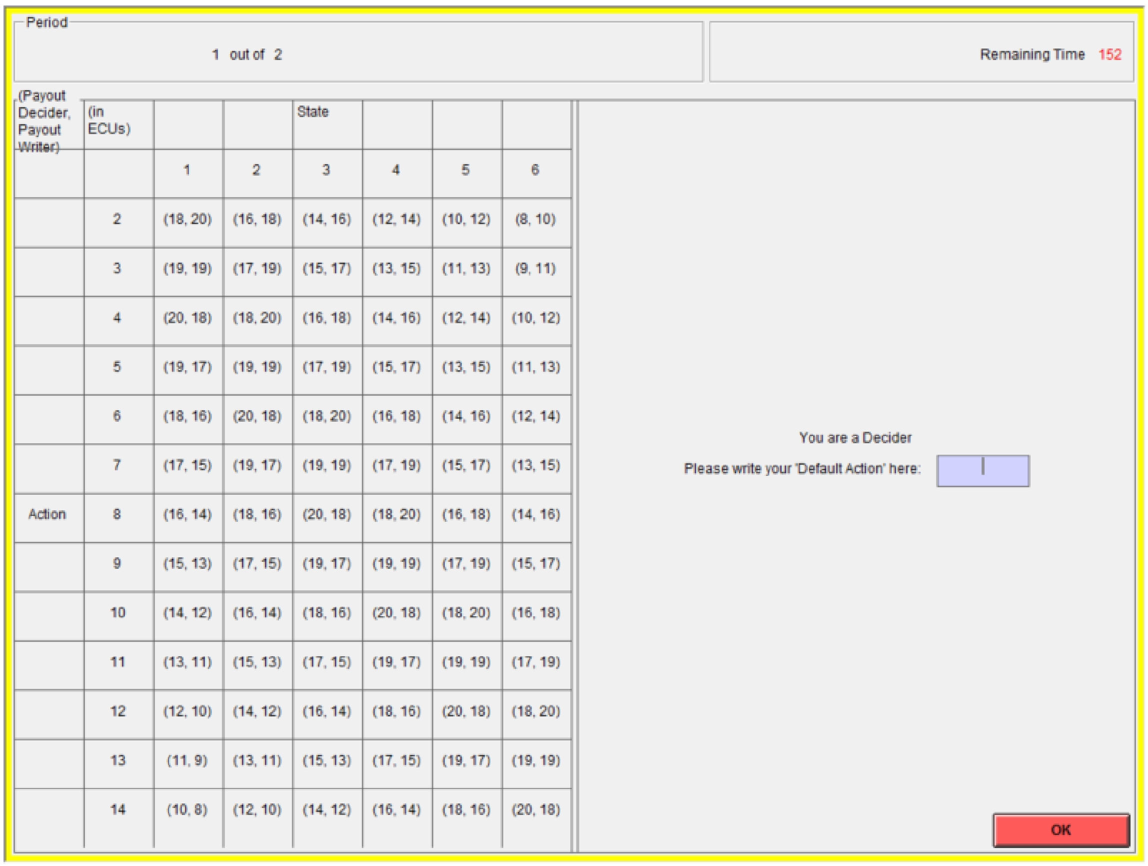
### 3.4.1 How Contracts are Written

In general, subjects wrote contracts that were on one edge of the state space according to Table 3.2. There appears to have been a move toward writing a contract on the edge of the state space as well (Figure 3.4), as in the last five rounds, 86% of people were writing contracts that contained state 1 or state 6. Across treatments there is little variation in this. Although this is still significantly different from 100% of people covering states 1 or 6 in their contract, a person who played randomly would have a .524 probability of contracting on an edge state (Since there are 21 possible $s_{low}$, $s_{high}$ combinations and 11 of those contain 1 or 6). Using a t-test, all play is significantly different from pure randomization except for in the Dfirst b=10 treatment for periods 6-10. This demonstrates that people tend to write contracts that include the bounds of the state space.




| (Payout Decider, Payout Writer) | (in ECUs) | State | | | | | |
|---|---|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 | 5 | 6 |
| | 2 | (18, 20) | (16, 18) | (14, 16) | (12, 14) | (10, 12) | (8, 10) |
| | 3 | (19, 19) | (17, 19) | (15, 17) | (13, 15) | (11, 13) | (9, 11) |
| | 4 | (20, 18) | (18, 20) | (16, 18) | (14, 16) | (12, 14) | (10, 12) |
| | 5 | (19, 17) | (19, 19) | (17, 19) | (15, 17) | (13, 15) | (11, 13) |
| | 6 | (18, 16) | (20, 18) | (18, 20) | (16, 18) | (14, 16) | (12, 14) |
| | 7 | (17, 15) | (19, 17) | (19, 19) | (17, 19) | (15, 17) | (13, 15) |
| Action | 8 | (16, 14) | (18, 16) | (20, 18) | (18, 20) | (16, 18) | (14, 16) |
| | 9 | (15, 13) | (17, 15) | (19, 17) | (19, 19) | (17, 19) | (15, 17) |
| | 10 | (14, 12) | (16, 14) | (18, 16) | (20, 18) | (18, 20) | (16, 18) |
| | 11 | (13, 11) | (15, 13) | (17, 15) | (19, 17) | (19, 19) | (17, 19) |
| | 12 | (12, 10) | (14, 12) | (16, 14) | (18, 16) | (20, 18) | (18, 20) |
| | 13 | (11, 9) | (13, 11) | (15, 13) | (17, 15) | (19, 17) | (19, 19) |
| | 14 | (10, 8) | (12, 10) | (14, 12) | (16, 14) | (18, 16) | (20, 18) |

Figure 3.1: The Writer Screen in Dfirst, b=2




| (Payout Decider, Payout Writer) | (in ECUs) | State | | | | | |
|---|---|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 | 5 | 6 |
| | 2 | (18, 20) | (16, 18) | (14, 16) | (12, 14) | (10, 12) | (8, 10) |
| | 3 | (19, 19) | (17, 19) | (15, 17) | (13, 15) | (11, 13) | (9, 11) |
| | 4 | (20, 18) | (18, 20) | (16, 18) | (14, 16) | (12, 14) | (10, 12) |
| | 5 | (19, 17) | (19, 19) | (17, 19) | (15, 17) | (13, 15) | (11, 13) |
| | 6 | (18, 16) | (20, 18) | (18, 20) | (16, 18) | (14, 16) | (12, 14) |
| | 7 | (17, 15) | (19, 17) | (19, 19) | (17, 19) | (15, 17) | (13, 15) |
| Action | 8 | (16, 14) | (18, 16) | (20, 18) | (18, 20) | (16, 18) | (14, 16) |
| | 9 | (15, 13) | (17, 15) | (19, 17) | (19, 19) | (17, 19) | (15, 17) |
| | 10 | (14, 12) | (16, 14) | (18, 16) | (20, 18) | (18, 20) | (16, 18) |
| | 11 | (13, 11) | (15, 13) | (17, 15) | (19, 17) | (19, 19) | (17, 19) |
| | 12 | (12, 10) | (14, 12) | (16, 14) | (18, 16) | (20, 18) | (18, 20) |
| | 13 | (11, 9) | (13, 11) | (15, 13) | (17, 15) | (19, 17) | (19, 19) |
| | 14 | (10, 8) | (12, 10) | (14, 12) | (16, 14) | (18, 16) | (20, 18) |

Figure 3.2: The Decider Screen in Dfirst, b=2

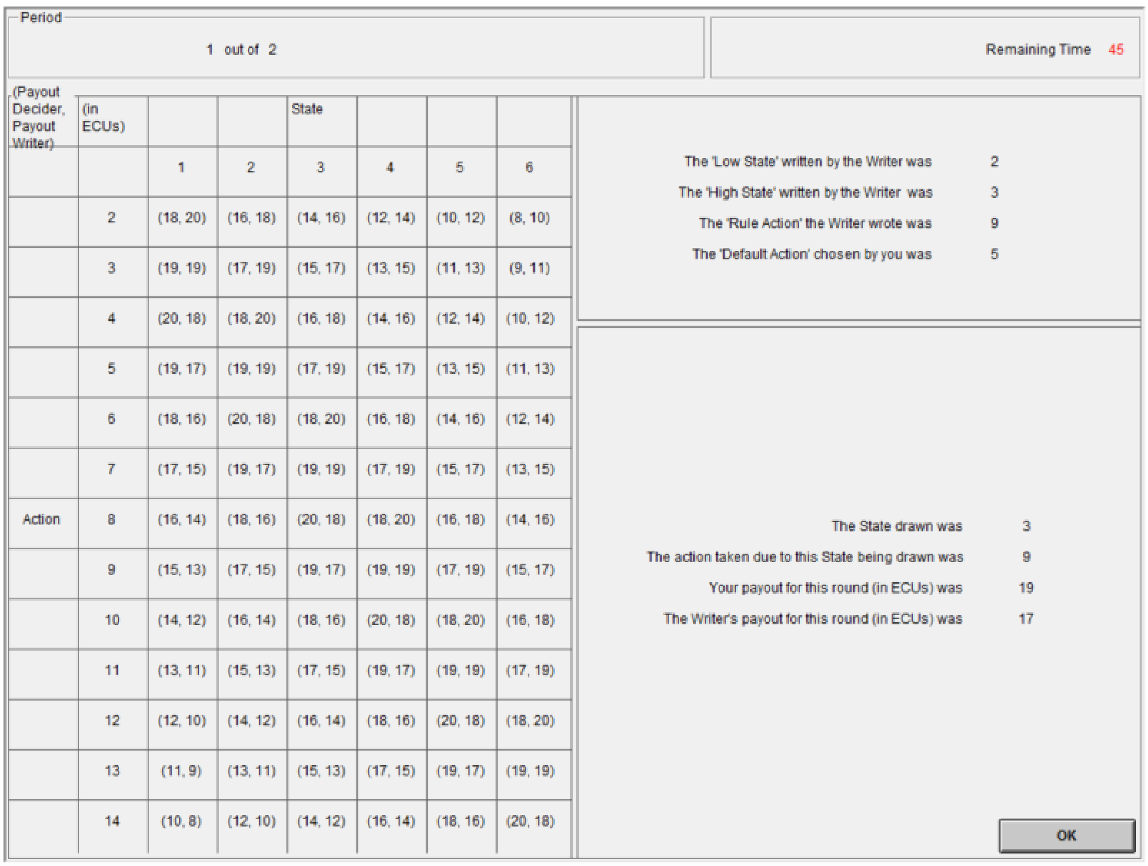



Figure 3.3: The Payout Screen for the Decider in Dfirst, b=2

*Result 1*: Prediction 1 is supported. A significant majority of the contracts written include $s_{low} = 1$ or $s_{high} = 6$. The number of contracts that include $s_{low} = 1$ or $s_{high} = 6$ is significantly more than random play.

For whether people write their actions optimally (Figure 3.5 and Table 3.3)), approximately 50% of rounds had contracts that included an optimal action given the states. Interestingly, there were a higher number of contracts written with an optimal action in the b=2 Wfirst treatment, which may have to do with the fact that in Wfirst, Writers could write their contracts optimally without having to worry about what the Deciders have done. In Wfirst b=10, in a fair number of periods, people wrote the contract $s_{low} = 1$, $s_{high} = 6$, $a_W = 10$, which is suboptimal, but in a way that helps the Decider in all states. In addition, there is a slight upward trend, as can be seen in figure 3.5. Statistically all treatments are different from pure randomization, with most specifications being different at the 1% level.

*Result 2*: Prediction 2 is supported. Subjects write rule actions that do statistically better than a subject who purely randomizes.

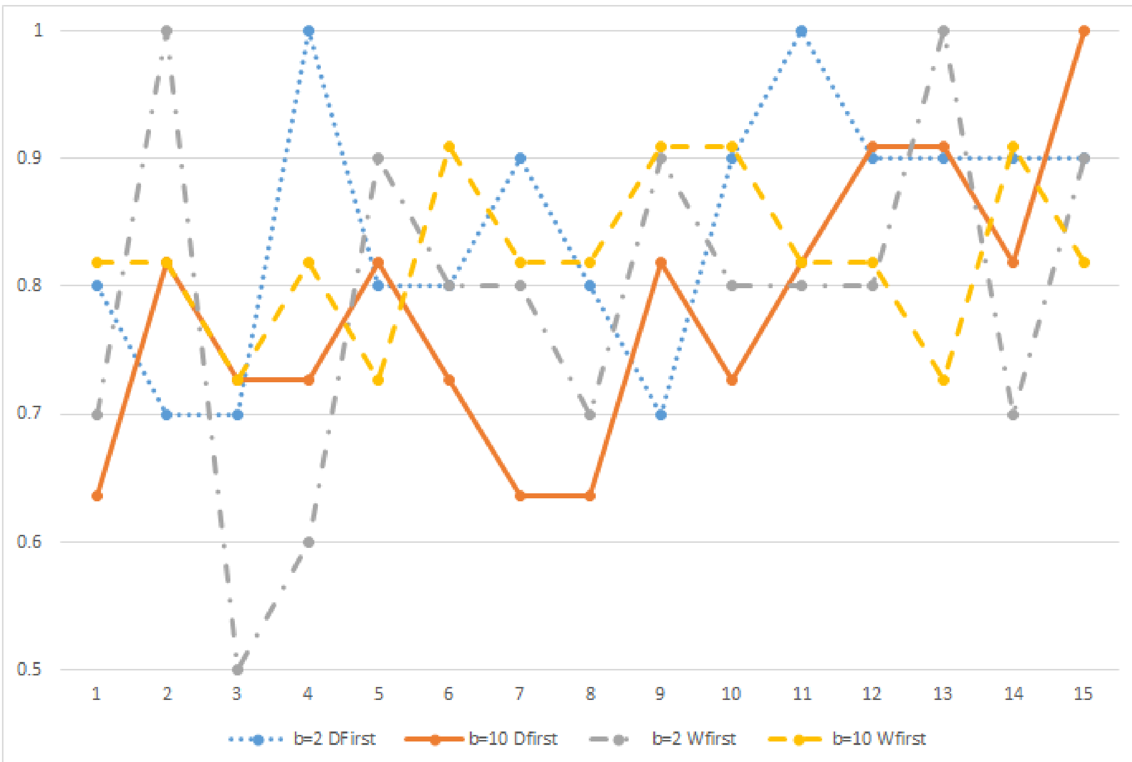



Figure 3.4: Rules That Cover States 1 or 6 by Period

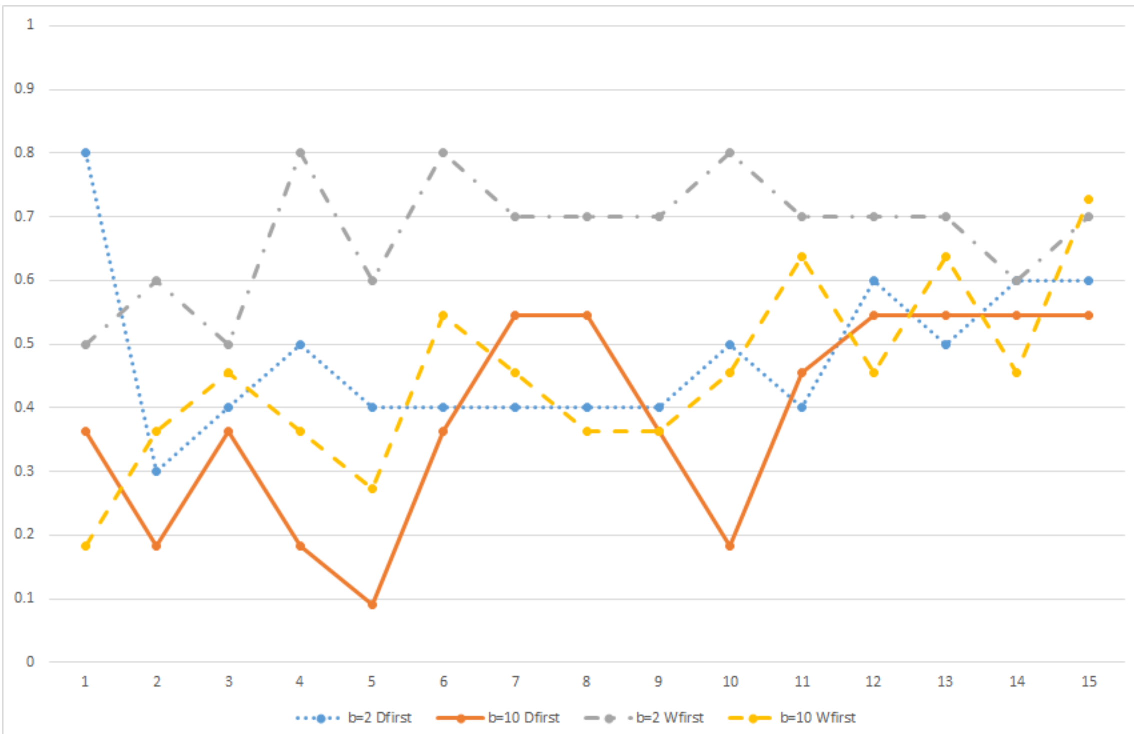



Figure 3.5: Fraction of Subjects Who Wrote Optimal Rule Actions Given Their Rules by Period

Table 3.2: Does the Contract Cover an Edge?

| Treatments | Contract Covers Edge | Periods 1-5 | Periods 6-10 | Periods 11-15 |
|---|---|---|---|---|
| Randomization Probability | | 0.524 | 0.524 | 0.524 |
| b=2, Dfirst | 0.847***$(n = 150)$ | 0.8***$(n = 50)$ | 0.82***$(n = 50)$ | 0.92***$(n = 50)$ |
| b=10, Dfirst | 0.782***$(n = 165)$ | 0.745***$(n = 55)$ | 0.709***$(n = 55)$ | 0.891***$(n = 55)$ |
| b=2, Wfirst | 0.793***$(n = 150)$ | 0.74***$(n = 50)$ | 0.8***$(n = 50)$ | 0.84***$(n = 50)$ |
| b=10, Wfirst | 0.824***$(n = 165)$ | 0.782***$(n = 55)$ | 0.873***$(n = 55)$ | 0.818***$(n = 55)$ |

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence intervals respectively using a one-sample t-test to test whether the indicated treatment has a significantly higher mean than is expected by random play.

Table 3.3: Do Subjects Write Rule Actions Optimally?

| Treatments | Action Optimal Given Rule | Periods 1-5 | Periods 6-10 | Periods 11-15 | Predicted Under Randomization |
|---|---|---|---|---|---|
| b=2, Dfirst | $0.48^{***}(n = 150)$ | $0.48^{***}(n = 50)$ | $0.42^{***}(n = 50)$ | $0.54^{***}(n = 50)$ | 0.25 |
| b=10, Dfirst | $0.448^{***}(n = 165)$ | $0.236^{*}(n = 55)$ | $0.4^{***}(n = 55)$ | $0.527^{***}(n = 55)$ | 0.15 |
| b=2, Wfirst | $0.673^{***}(n = 150)$ | $0.6^{***}(n = 50)$ | $0.74^{***}(n = 50)$ | $0.68^{***}(n = 50)$ | 0.25 |
| b=10, Wfirst | $0.448^{***}(n = 165)$ | $0.327^{***}(n = 55)$ | $0.436^{***}(n = 55)$ | $0.582^{***}(n = 55)$ | 0.15 |

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence intervals respectively using a one-sample t-test to test whether the indicated treatment has a significantly higher mean than is expected by random play.

Table 3.4: Number of States in Each Contract

| Treatments | Average Number of States | Periods 1-5 | Periods 6-10 | Periods 11-15 |
|---|---|---|---|---|
| b=2, Dfirst | 3.92 (n=150) | 4.04 (n=50) | 3.64 (n=50) | 4.08 (n=50) |
| b=10, Dfirst | 4.461 (n=165) | 4.109 (n=55) | 4.382 (n=55) | 4.891 (n=55) |
| t-Stat for difference between b=2 and b=10 | -3.063*** | -0.218 | -2.464*** | -2.795*** |
| b=2, Wfirst | 4.053 (n=150) | 4.18 (n=50) | 4.04 (n=50) | 3.94 (n=50) |
| b=10, Wfirst | 4.879 (n=165) | 4.564 (n=55) | 5.091 (n=55) | 4.982 (n=55) |
| t-Stat for difference between b=2 and b=10 | -4.485*** | -1.176 | -3.283*** | -3.357*** |
| All | 0.811 | 0.767 | 0.8 | 0.867 |

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence intervals respectively using a two-sample t-test for differences in means.

In periods 1-5, the number of states covered by contracts (Figure 3.6 and Figure 3.7) are statistically indistinguishable. However, in periods 6-10 and 11-15, there is a large separation in both Dfirst and Wfirst. This is strong evidence of a learning effect. In both treatments, periods 6-10 and 11-15 have t-statistics that indicate significant difference at the 5% level, which can be observed in table 4.[2]

*Result 3*: Prediction 3 is supported. The number of states covered in each contract in the $b = 2$ treatment is significantly different from the $b = 10$ treatment.

[2]In addition, a Z-test was run with clustered standard errors to combat any doubt about independence across periods for the same individual. Unfortunately, the significance disappears (becomes significant at the 15% level). However, when the treatments are pooled, the two periods are still significantly different. This is likely due to the smallness of the sample size.

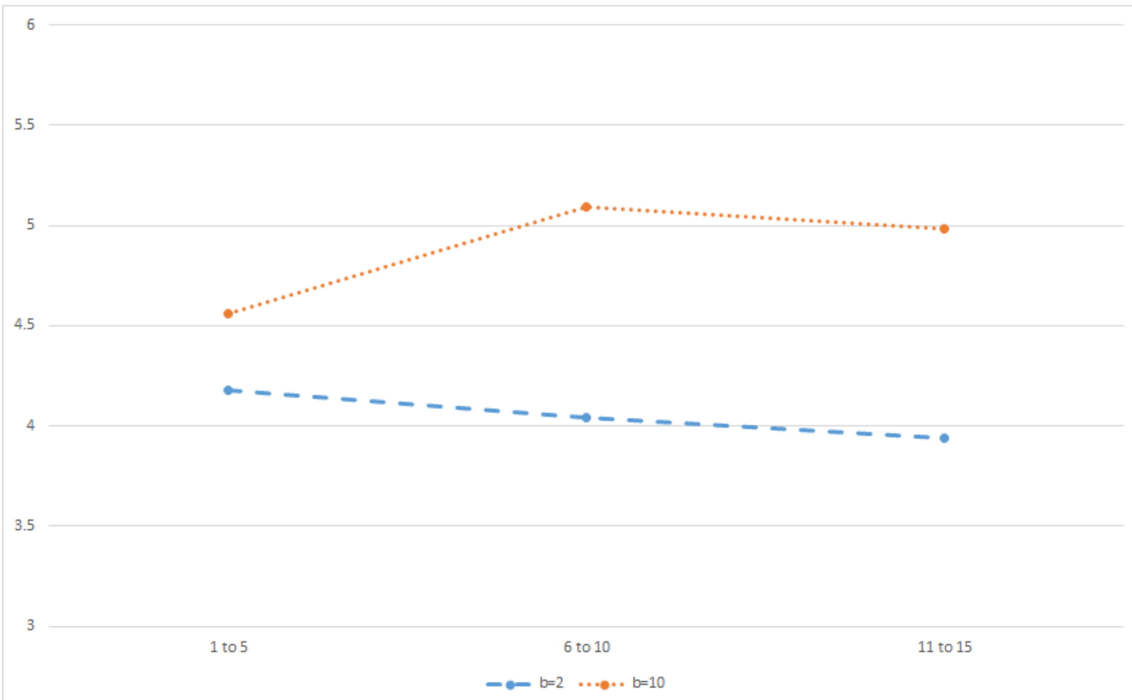



Figure 3.6: Number of States Covered by a Contract in Wfirst for b=2 and b=10, Including the Predicted SPE

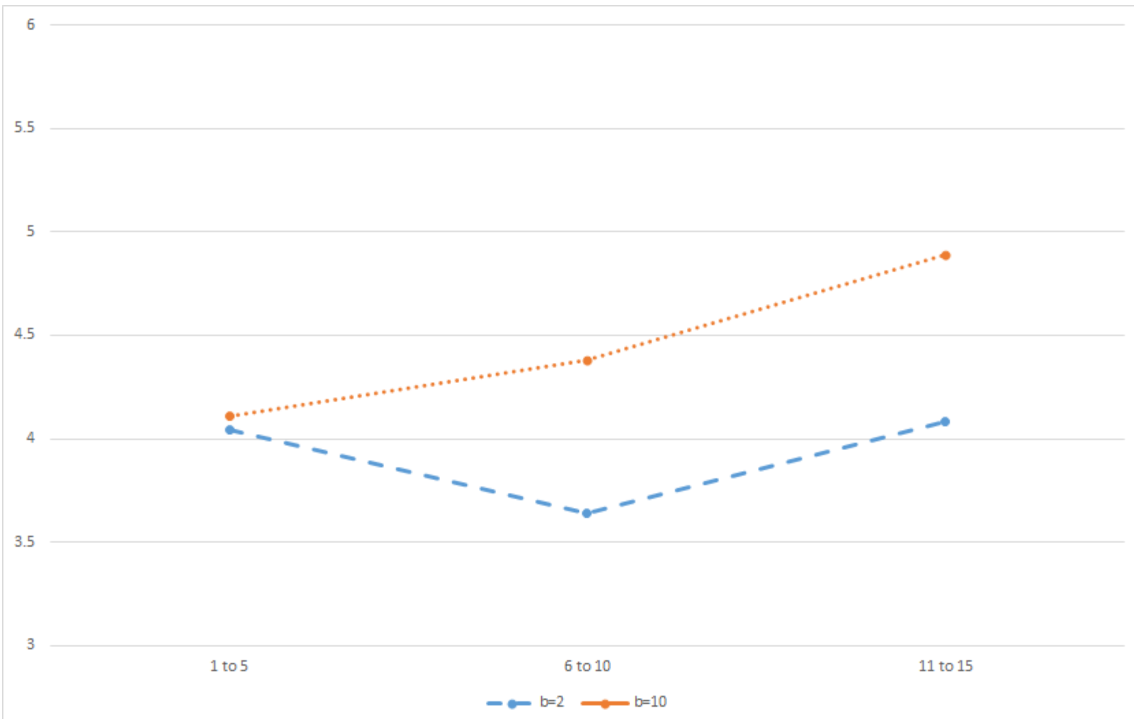
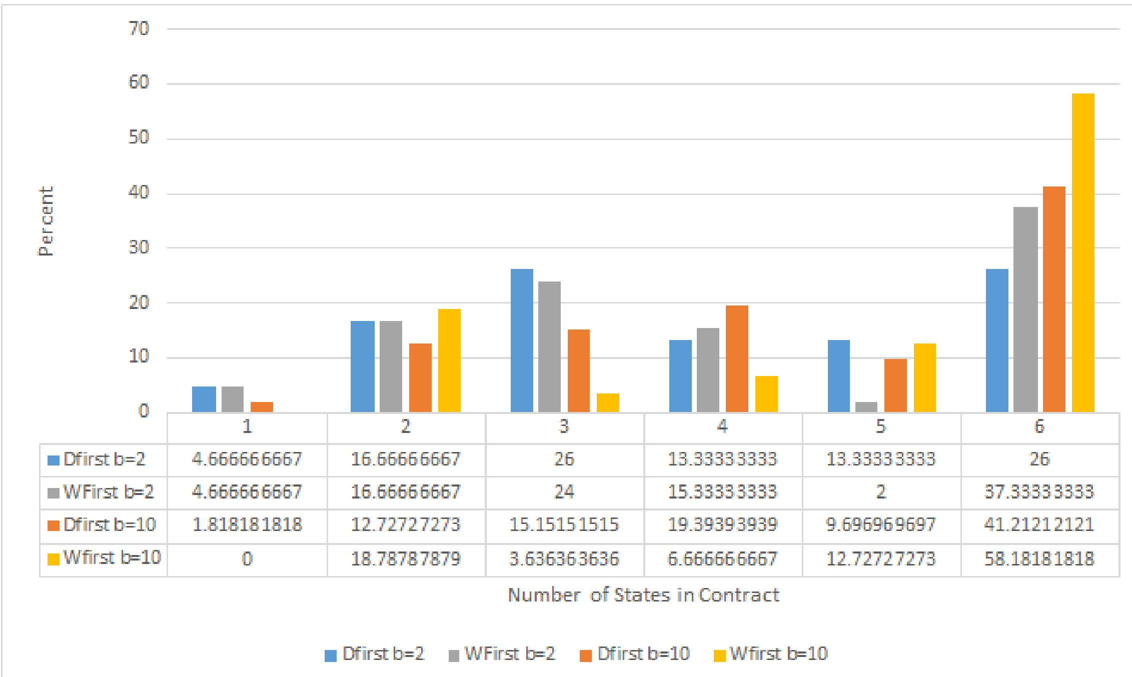



Figure 3.7: Number of States Covered by a Contract in Dfirst for b=2 and b=10, Including the Predicted SPE




| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Dfirst b=2 | 4.666666667 | 16.66666667 | 26 | 13.33333333 | 13.33333333 | 26 |
| WFirst b=2 | 4.666666667 | 16.66666667 | 24 | 15.33333333 | 2 | 37.33333333 |
| Dfirst b=10 | 1.818181818 | 12.72727273 | 15.15151515 | 19.39393939 | 9.696969697 | 41.21212121 |
| Wfirst b=10 | 0 | 18.78787879 | 3.636363636 | 6.666666667 | 12.72727273 | 58.18181818 |

Figure 3.8: Distribution of the Number of States in a Contract over All Treatments

Table 3.5: In Dfirst, Are Decider Choices Possible SPE?

| Treatments | Action Optimal Given Rule | Periods 1-5 | Periods 6-10 | Periods 11-15 | Predicted Under Randomization |
|---|---|---|---|---|---|
| b=2, Dfirst | $0.269 (n = 150)$ | $0.190 (n = 50)$ | $0.26 (n = 50)$ | $0.44 (n = 50)$ | 0.583 |
| b=10, Dfirst | $0.636^{***} (n = 165)$ | $0.655^{**} (n = 55)$ | $0.618^{**} (n = 55)$ | $0.636^{**} (n = 55)$ | 0.5 |

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence intervals respectively using a one-sample t-test to test whether the indicated treatment has a significantly higher mean than is expected by random play.

Table 3.6: In Wfirst, are $s_{low}$ and $s_{high}$ consistent with SPE?

| Treatments | Action Optimal Given Rule | Periods 1-5 | Periods 6-10 | Periods 11-15 | Predicted Under Randomization |
|---|---|---|---|---|---|
| b=2, Wfirst | $0.373^{**} (n = 150)$ | $0.28 (n = 50)$ | $0.38^{*} (n = 50)$ | $0.46^{***} (n = 50)$ | 0.286 |
| b=10, Wfirst | $0.612^{***} (n = 165)$ | $0.545^{***} (n = 55)$ | $0.655^{***} (n = 55)$ | $0.636^{***} (n = 55)$ | 0.095 |

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% confidence intervals respectively using a one-sample t-test to test whether the indicated treatment has a significantly higher mean than is expected by random play.

Table 3.5, displays whether Deciders who move first have play consistent with SPE, while Table 3.6 shows the same test for Writers who move first. Writers in the Wfirst $b = 10$ treatment and Deciders in the Dfirst $b = 10$ treatment chose actions that align with SPE predictions far more often than would be predicted by random play. Writers in the Wfirst $b = 2$ treatment write contracts that do better than a random subject. However Deciders in the Dfirst $b = 2$ treatment do far worse than a random player, although the trend is in the correct direction over time. As can be seen in figure 3.9, 38% of Deciders pick a default action of 8. This, coupled with the fact that 27% of Deciders pick a default action of 12 in the Dfirst $b = 10$ treatment (Figure 3.10), indicate that some bias is influencing the way that Deciders choose default actions.

*Result 4*: Prediction 4 is supported for Writers and Deciders in the Dfirst, $b = 10$ treatment. Writers write contracts that align with predictions far more frequently than random play would suggest. Prediction 4 is not supported for Deciders in the Dfirst, $b = 2$ treatment. Although more Deciders select an equilibrium default action over time, Deciders select fewer default actions that are consistent with predictions than a completely random player.

One aspect of these results worth discussing is that across all of the findings, there is some evidence for learning taking place, which is seen for the Writers in Figures 3.6, and 3.7 and for the Deciders in figures 3.11 and 3.12. This game has a high degree of complexity, so it is encouraging that there seems to be a learning effect. The following section will examine whether play is in accordance with a level-k model.

### 3.4.2 Behavioral Preferences

In many comments on the post-experimental questionnaire, subjects mentioned fairness when discussing their view of the game. Subjects wrote about working together to create outcomes that were good for both parties and sometimes discussed the Writer having too much control. In addition Deciders and Writers write suboptimal default actions and contracts when they move first. This indicates that analysis level-k models may be fruitful.

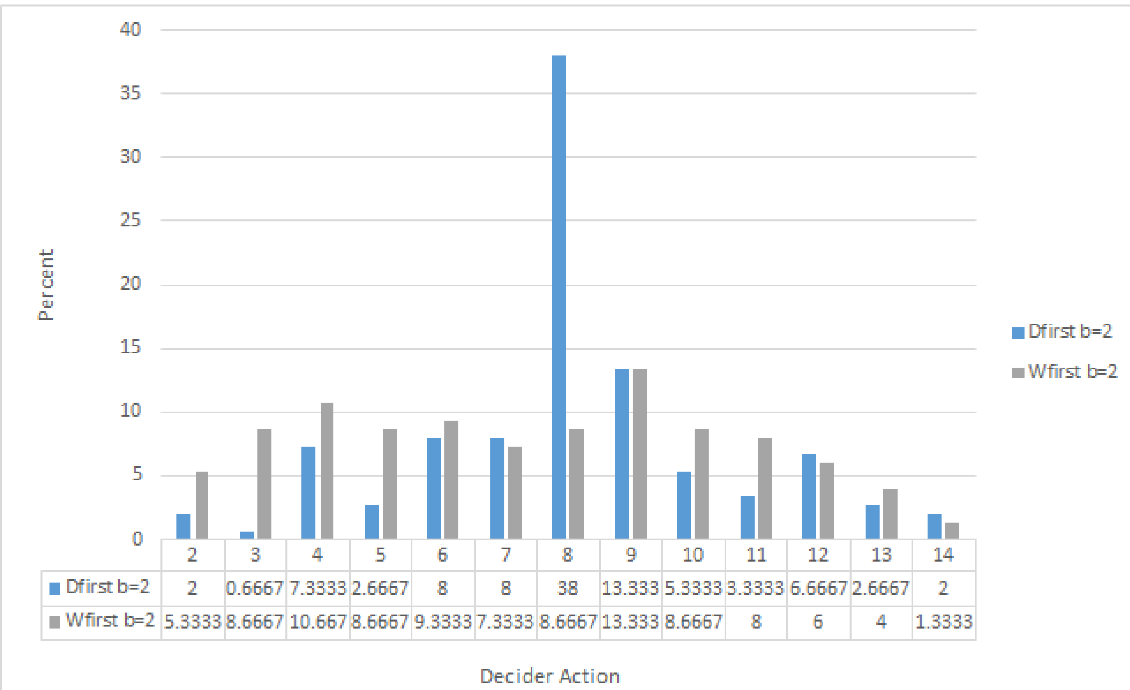
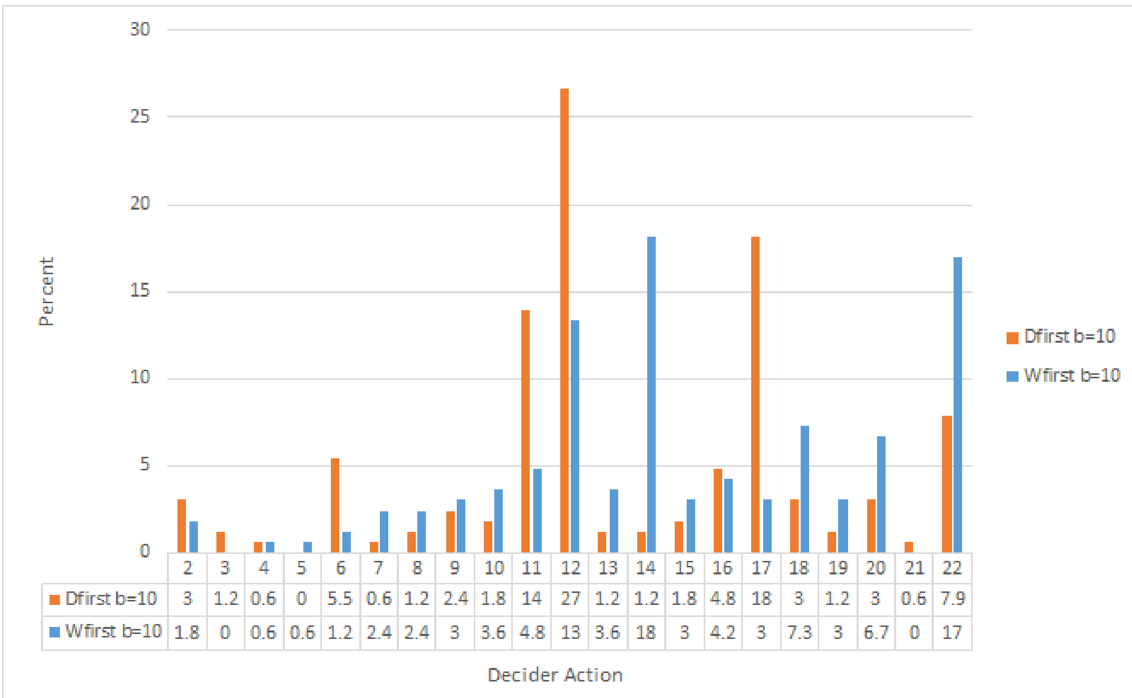
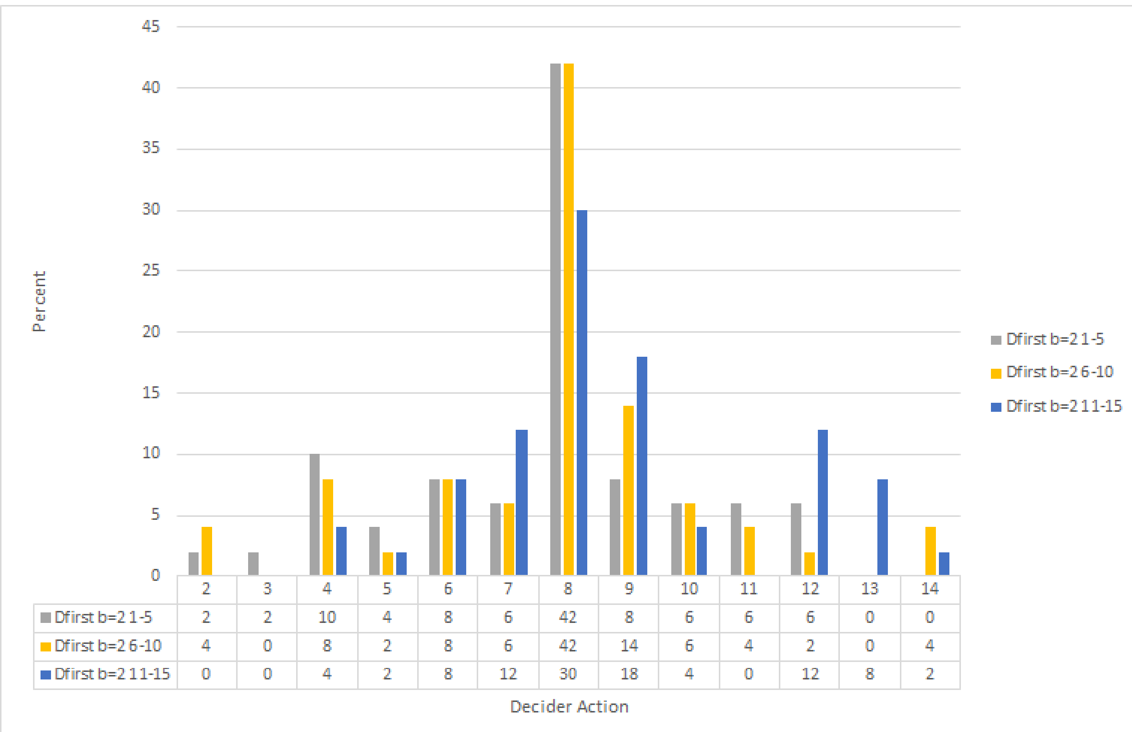



Figure 3.9: Distribution of Decider Actions in the b=2 Treatment



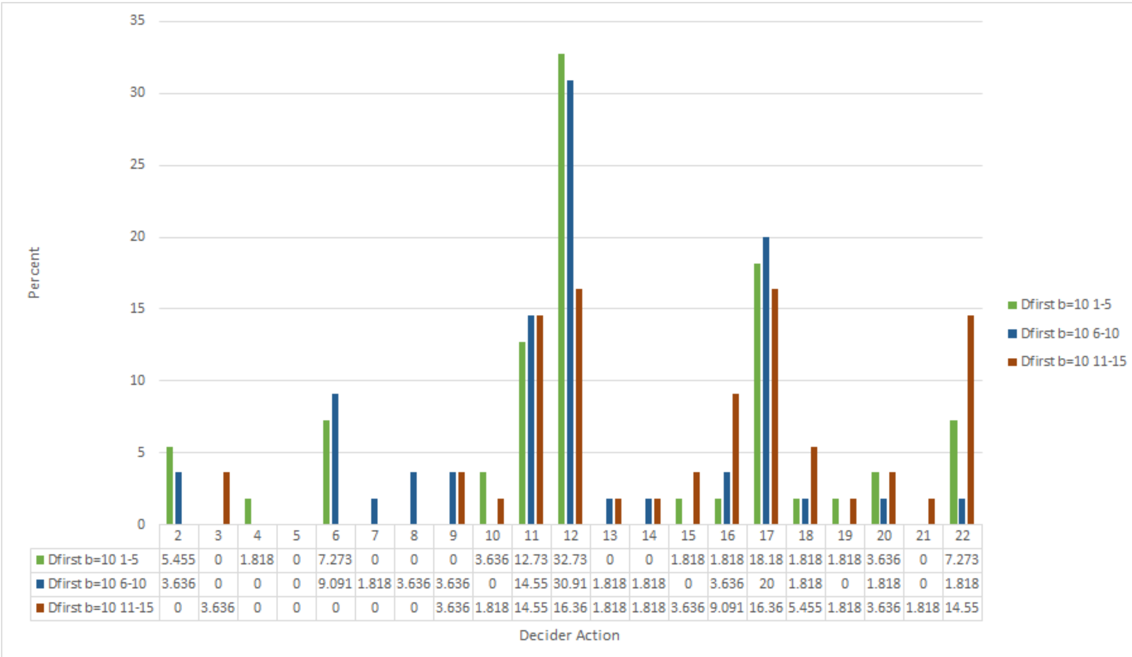

| | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dfirst b=10 | 3 | 1.2 | 0.6 | 0 | 5.5 | 0.6 | 1.2 | 2.4 | 1.8 | 14 | 27 | 1.2 | 1.2 | 1.8 | 4.8 | 18 | 3 | 1.2 | 3 | 0.6 | 7.9 |
| Wfirst b=10 | 1.8 | 0 | 0.6 | 0.6 | 1.2 | 2.4 | 2.4 | 3 | 3.6 | 4.8 | 13 | 3.6 | 18 | 3 | 4.2 | 3 | 7.3 | 3 | 6.7 | 0 | 17 |

Figure 3.10: Distribution of Decider Actions in the b=10 Treatment




| | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dfirst b=2 1-5 | 2 | 2 | 10 | 4 | 8 | 6 | 42 | 8 | 6 | 6 | 6 | 0 | 0 |
| Dfirst b=2 6-10 | 4 | 0 | 8 | 2 | 8 | 6 | 42 | 14 | 6 | 4 | 2 | 0 | 4 |
| Dfirst b=2 11-15 | 0 | 0 | 4 | 2 | 8 | 12 | 30 | 18 | 4 | 0 | 12 | 8 | 2 |

Figure 3.11: Distribution of Decider Actions by Period in b=2 Treatment




| | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dfirst b=10 1-5 | 5.455 | 0 | 1.818 | 0 | 7.273 | 0 | 0 | 0 | 3.636 | 12.73 | 32.73 | 0 | 0 | 1.818 | 1.818 | 18.18 | 1.818 | 1.818 | 3.636 | 0 | 7.273 |
| Dfirst b=10 6-10 | 3.636 | 0 | 0 | 0 | 9.091 | 1.818 | 3.636 | 3.636 | 0 | 14.55 | 30.91 | 1.818 | 1.818 | 0 | 3.636 | 20 | 1.818 | 0 | 1.818 | 0 | 1.818 |
| Dfirst b=10 11-15 | 0 | 3.636 | 0 | 0 | 0 | 0 | 0 | 3.636 | 1.818 | 14.55 | 16.36 | 1.818 | 1.818 | 3.636 | 9.091 | 16.36 | 5.455 | 1.818 | 3.636 | 1.818 | 14.55 |

Figure 3.12: Distribution of Decider Actions by Period in b=10 Treatment

**Level-k**

One model that can help explain play in complex games is a level-k model. The starting point this paper will use for level-0 is that the Decider and Writer both randomize over all actions for level 0. Level 1 for the Decider is a best response to the Writer's level 0, level 1 for the Writer is a best response to the Decider's level 0, and so on. This starting point seems the most sensible, as players have many things to consider and may just pick an action that is good against a random selection of the other players. The $\frac{3}{5}$ rule is effective for fitting people into levels: If, in the last 10 periods of the game, at least $\frac{3}{5}$ of a subject's play corresponded to a certain level, then they will be classified as that level. In addition, for Writers, I will allow them an error of 1 away from the Writer Action that fits into a level. This means that, for example, a Writer in the $b = 2$ treatment who writes the contract $(1, 2, 5)$ would be classified as a level 2 rather than a level 0 in that period. In tables 6 and 7, after the first and second level, the third level looks similar to the first level. For this reason, I will only focus on levels 0 through 2.

Table 3.7: Table 6: Levels in b=2

| | Writer Contract $(s_{low}, s_{high}, a_W)$ | Decider Action |
|---|---|---|
| Level 0 | Randomize uniformly between all possible contracts | Randomize uniformly between $\{4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14\}$ |
| Level 1 | $\{((1,3),4)\}$ | $\{8, 9, 10\}$ |
| Level 2 | $\{((1,2),2),((1,2),3),((1,2),4),((1,3),4)\}$ | $\{10\}$ |
| Level 3 | $\{((1,3),4)\}$ | $\{8, 9, 10\}$ |

Table 3.8: Levels in b=10

| | Writer | Decider |
|---|---|---|
| Level 0 | Randomize uniformly between all possible contracts | Randomize uniformly between $\{12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22\}$ |
| Level 1 | $\{((1, 5), 6)\}$ | $\{16, 17, 18\}$ |
| Level 2 | $\{((1, 5), 6), ((1, 6), 5), ((1, 6), 6), ((1, 6), 7)\}$ | $\{22\}$ |
| Level 3 | $\{((1, 6), 5), ((1, 6), 6), ((1, 6), 7)\}$ | $\{22\}$, Randomize uniformly between $\{12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22\}$ |

Table 3.9: Results of Level-k Analysis When Player Moves First

| | Writers in level in $b = 2$ | Deciders in level in $b = 2$ | Writers in level in $b = 10$ | Deciders in level in $b = 10$ |
|---|---|---|---|---|
| Level 0 | 8 | 5 | 8 | 6 |
| Level 1 | 1 | 5 | 1 | 2 |
| Level 2 | 1 | 0 | 2 | 0 |
| Total Number of Subjects | 10 | 10 | 11 | 11 |

Table 3.10: Results of Level-k Analysis When Player Moves Second

| | Writers in level in $b = 2$ | Deciders in level in $b = 2$ | Writers in level in $b = 10$ | Deciders in level in $b = 10$ |
|---|---|---|---|---|
| Level 0 | 7 | 8 | 7 | 9 |
| Level 1 | 1 | 1 | 0 | 1 |
| Level 2 | 2 | 1 | 4 | 0 |
| Total Number of Subjects | 10 | 10 | 11 | 11 |

For the results of the level-k analysis, which can be seen in Tables 3.9 and 3.10, the results immediately point away from level-k being explanatory. Firstly, in the $b = 2$ and $b = 10$ treatments, if level-k held it is expected that first movers in both treatments would appear to be similar in the data. Secondly, far too much of the data is explained by level 0, which is where most of the subjects are classified since they don't tend to play according to the levels.[3] This indicates that level-k does not explain behavior in this setting.

*Result 5*: This model of level-k does not explain behavior in this experiment, as many of the subjects are classified as level-0 which coincides with random play.

## 3.5 Conclusion

This paper proposes an experiment that studies how incomplete contracts are written and how well theory predicts what interpretive rules and contracts are played in a laboratory setting. Subjects tend to write their contracts correctly in many ways. Writer actions, contract length, and the location of the contract are all played in accordance with theory. Additionally there is some evidence that first movers select actions that are consistent with subgame perfect equilibrium.

One factor that goes against predictions is that default actions for Deciders who move first are not focused on equilibrium. Additionally, in the post-experimental questionnaire, subjects indicated that fairness may be at play. This would make the out-of-equilibrium play make sense, as play by Deciders when they were first movers was focused around actions right in the middle of what was possible. One way to test this would be to rerun the experiment with different possible actions, as this could be an unconscious bias towards default actions in the middle of the state space.

In total, there is room for further exploration involving how people learn to write these contracts and default rules, since there is evidence that subjects learn how to take actions that have characteristics predicted in equilibrium. The findings presented in this paper indicate that there is more exploration necessary to grasp the full picture that links the intentionally incomplete contracting literature to how

[3] Some of the subjects are classified as nothing in the $b = 10$ treatment where Deciders played actions below 12 a majority of the time.

contracts may be written by human subjects.

# Appendix for Chapter 1

## 3.6 Appendix A1: Misc Tables and Graphs

In this section I will include tables and graphs that are not directly relevant to the main text that may be of interest.

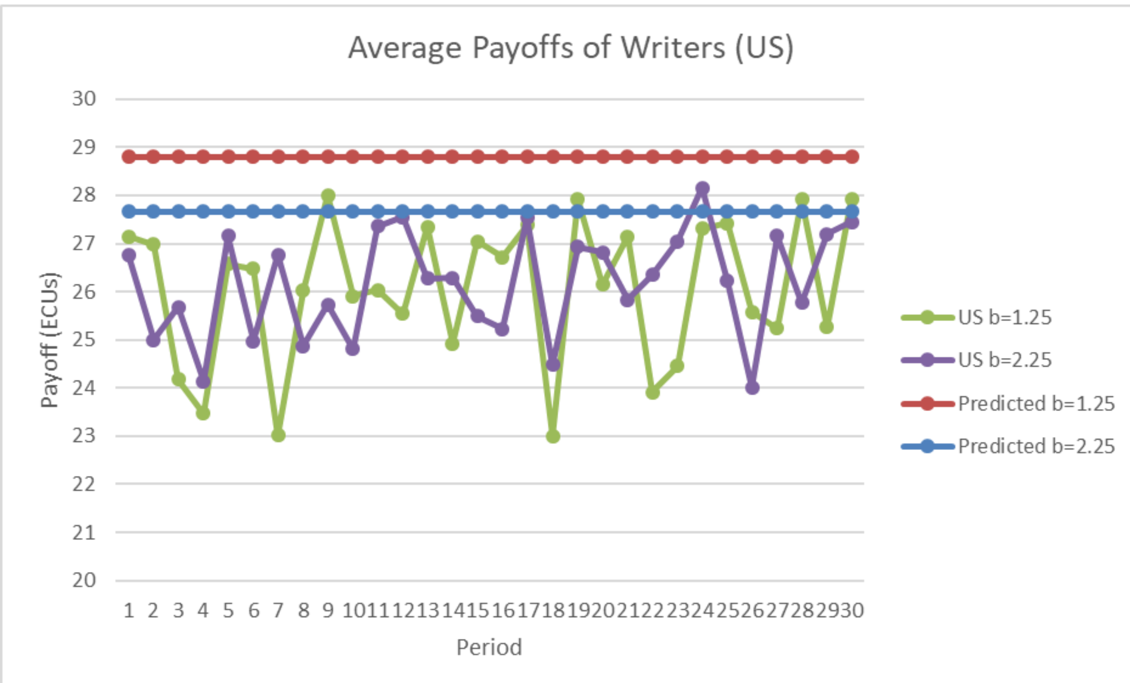



Figure 3.13: Average Writer Payoff of US Subjects

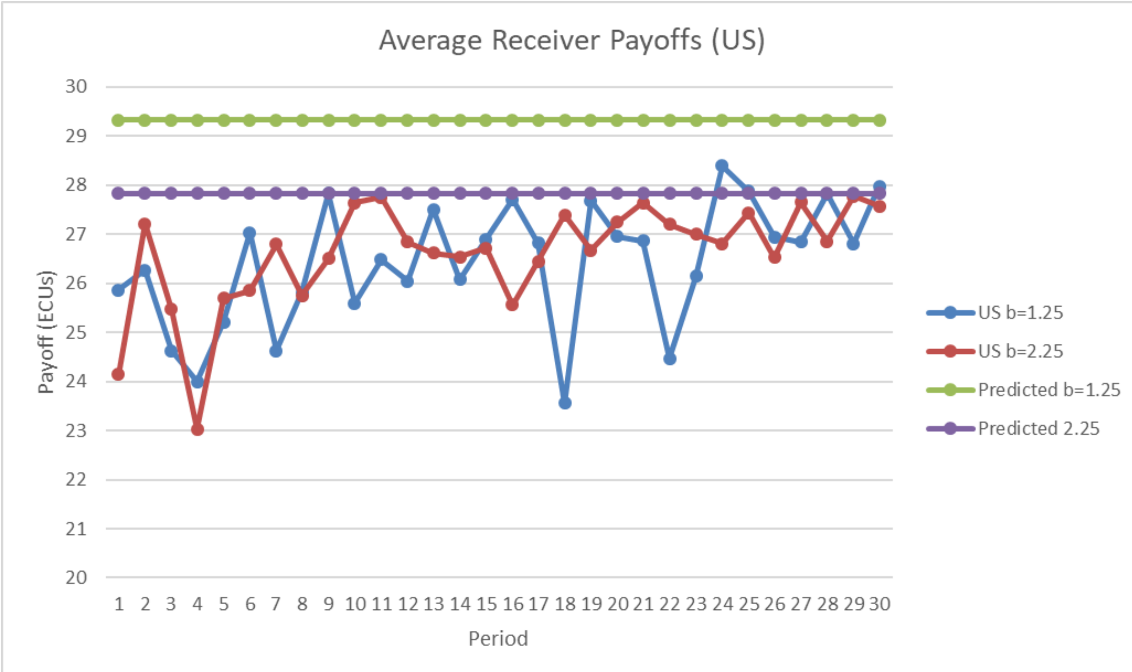



Figure 3.14: Average Receiver Payoff of US Subjects

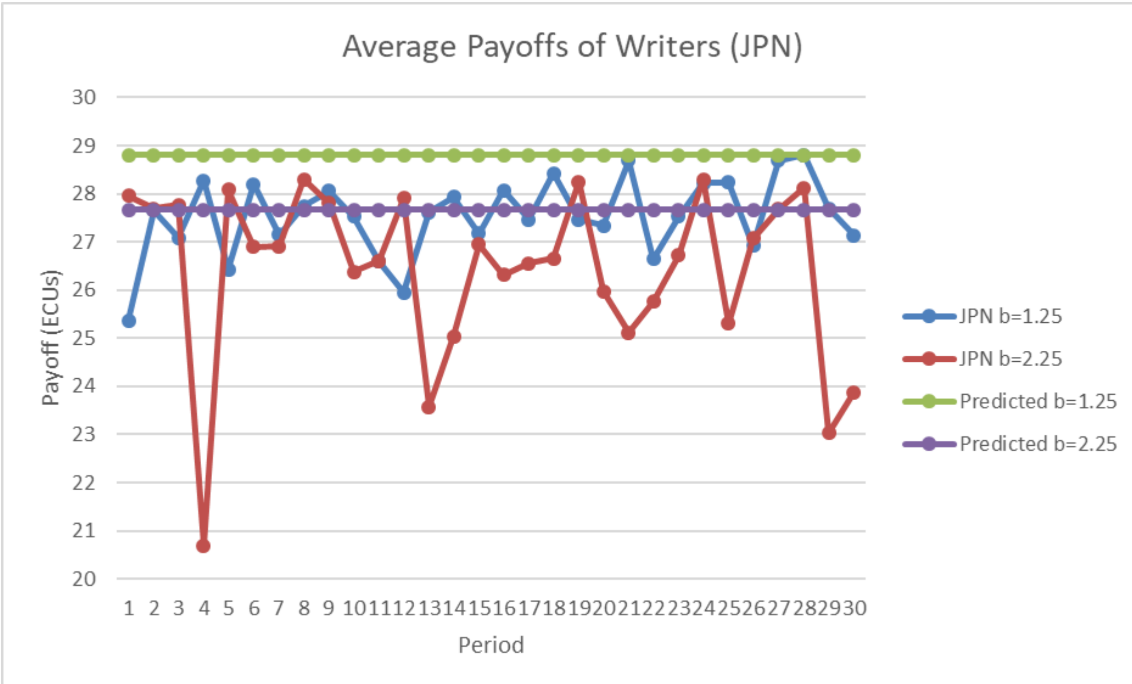



Figure 3.15: Average Writer Payoff of JPN Subjects

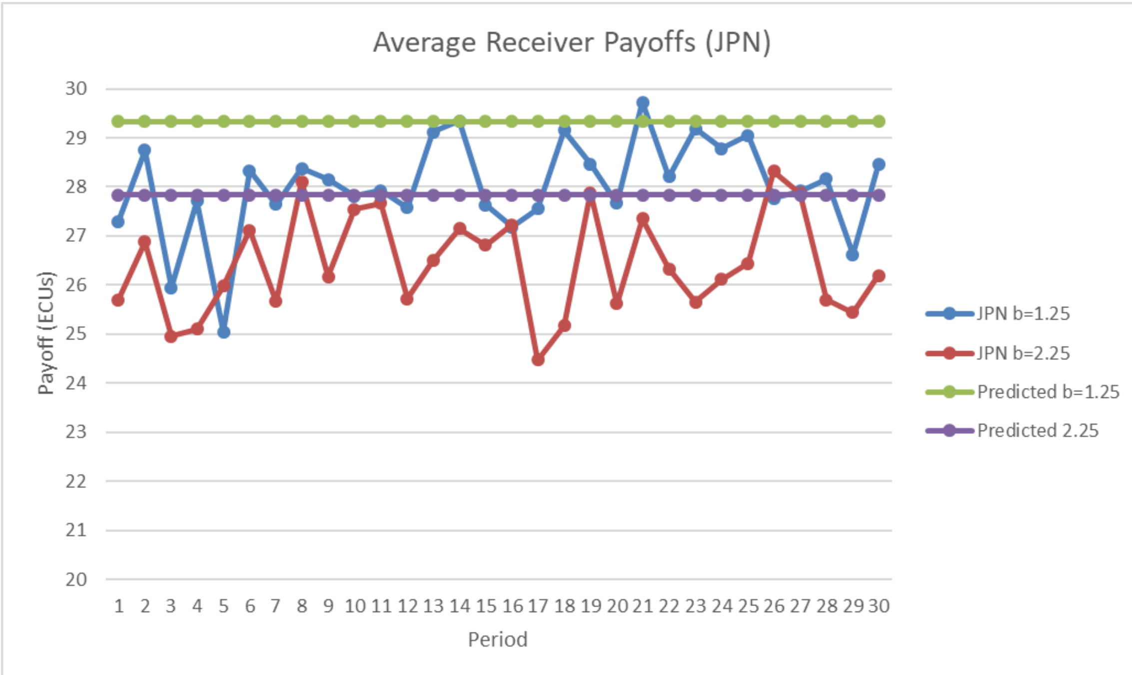



Figure 3.16: Average Receiver Payoff of JPN Subjects

Table 3.11: Writer Payoff Given Communication Subgame is Reached

| | b=1.25 | | b=2.25 | |
|---|---|---|---|---|
| (Low State, High State) | Average Payoff | Predicted Payoff | Average Payoff | Predicted Payoff |
| No Contract | 27.4 | 26.438 | 26.066 | 20.938 |
| (1,1) | 25.526 | 26.438 | 25.072 | 19.938 |
| (3,3) | 24.3 | 27.938 | 24.250 | 22.938 |
| (5,5) | 27.573 | 27.438 | 25.399 | 23.938 |
| (7,7) | 29.19 | 27.938 | 26.109 | 22.938 |
| (9,9) | 27.476 | 26.438 | 27.442 | 19.938 |
| (1,3) | 26.178 | 27.771 | 23.423 | 22.271 |
| (3,5) | 24.269 | 27.771 | 24.964 | 24.271 |
| (5,7) | 23.978 | 27.771 | 26.918 | 24.271 |
| (7,9) | 28.184 | 27.771 | 27.871 | 22.271 |
| (1,5) | 26.635 | 27.438 | 23.598 | 23.938 |
| (3,7) | 24.206 | 28.438 | 25.444 | 24.938 |
| (5,9) | 25.87 | 27.438 | 26.621 | 23.938 |
| (1,7) | 22.47 | 28.438 | 20.674 | 24.938 |
| (3,9) | 26.376 | 28.438 | 27.384 | 24.938 |
| (1,9) | N/A | N/A | N/A | N/A |

Table 3.12: Receiver Payoff Given Communication Subgame is Reached

| | b=1.25 | | b=2.25 | |
|---|---|---|---|---|
| (Low State, High State) | Average Payoff | Predicted Payoff | Average Payoff | Predicted Payoff |
| No Contract | 27.847 | 28 | 26.790 | 26 |
| (1,1) | 27.089 | 28 | 27.537 | 25 |
| (3,3) | 25.958 | 29.5 | 27.213 | 28 |
| (5,5) | 27.222 | 29 | 27.396 | 29 |
| (7,7) | 28.986 | 29.5 | 27.74 | 28 |
| (9,9) | 28.068 | 28 | 27.198 | 25 |
| (1,3) | 27.451 | 29.333 | 26.819 | 27.333 |
| (3,5) | 25.066 | 29.333 | 27.932 | 29.333 |
| (5,7) | 25.052 | 29.333 | 27.129 | 29.333 |
| (7,9) | 26.914 | 29.333 | 27.427 | 27.333 |
| (1,5) | 26.718 | 29 | 27.319 | 29 |
| (3,7) | 24.797 | 30 | 26.629 | 30 |
| (5,9) | 24.3 | 29 | 26.18 | 29 |
| (1,7) | 24.573 | 30 | 25.6 | 30 |
| (3,9) | 24.973 | 30 | 27.696 | 30 |
| (1,9) | N/A | N/A | N/A | N/A |

## 3.7 Appendix B1: Proofs for Observations

Each observation will be stated below for convenience:

**Observation 1:** *Given a low state $s_{low}$ and a high state $s_{high}$, the optimal contract specifies the writer action $a_w = \frac{s_{low}+s_{high}}{2} + b$. Conditional on the receiver knowing that the state $s \in S' \subseteq S$, the optimal receiver action is $\mathbb{E}[s \mid s \in S']$.*

**Proof.** The first statement is proved by writing down the first order condition for the expected utility problem and noting that the expected utility is concave. For a contract $(s_{low}, s_{high}, a^*)$

$$EU_W(\cdot) = \sum_{0}^{\frac{s_{high}-s_{low}}{2}} -\frac{1}{\frac{s_{high}-s_{low}}{2}+1}\left(s_{low}+2i+b-a^*\right)^2.$$

Taking the first derivative and setting it equal to 0 yields the equation

$$\sum_{0}^{\frac{s_{high}-s_{low}}{2}} \frac{2}{\frac{s_{high}-s_{low}}{2}}\left(s_{low}+2i+b-a^*\right) = 0.$$

Solving for $a^*$ reduces the equation to

$$a^* = s_{low} + b + \frac{\sum_{1}^{\frac{s_{high}-s_{low}}{2}} 2i}{\frac{s_{high}-s_{low}}{2}+1} = s_{low} + b + \frac{\frac{2\left(\frac{s_{high}-s_{low}}{2}\right)\left(\frac{s_{high}-s_{low}}{2}+1\right)}{2}}{\frac{s_{high}-s_{low}}{2}+1}$$

$$= s_{low} + b + \frac{s_{high}-s_{low}}{2} = \frac{s_{high}+s_{low}}{2} + b.$$

The second statement is trivially true because the receiver is strictly risk averse and the payoff function is symmetric. □

**Observation 2:** *In any communication subgame, a perfect Bayesian equilibrium of that subgame is represented as a partition $P = \{p_1, \ldots, p_n\}$ for $n \geq 1$, where $p_i = \left\{s_1^i, ..., s_{k_i}^i\right\}$ is a partition element such that a message $m \in M_i \subseteq M$ sent when $s^i \in p_i$ induces a unique expected action $a_R^i \neq a_R^j \forall j$, where $a_R^i$ is the expected receiver* *preferred action given $m \in M_i$ is sent. Each partition element is ordered such that for any $s^i \in p_i$, $s^j \in p^j$, $s^i < s^j$.*

**Proof.** In any perfect Bayesian equilibrium of any communication subgame, for some subset of the message space $\hat{M}$ a writer must weakly prefer sending a message $m \in \hat{M}$ in state $s$ to sending any other message $m' \in \left\{M \setminus \hat{M}\right\}$, given that a receiver best responds by playing the action $a_R = \mathbb{E}\left[s \mid m\right]$. Denote a set of states that induce the same expected action $p_i$. These $p_i$ form the partition $P = \{p_1, \ldots, p_n\}$. Denote the set of messages that can possibly be sent if $s \in p_i$ by $M_i$. Because the actions in any two partition elements differ and because the single crossing condition holds, for $a_j > a_i$ if a writer prefers action $a_i$ in state $s \in p_i$ and a writer prefers action $a_j$ in state $s' \in p_j$, then for any $s'' > s'$ a writer must prefer $a_j$ to $a_i$, meaning that $p_i$ cannot contain any states larger than any state in $p_j$. $\square$

**Observation 3:** *A writer will always choose to write a contract.*

**Proof.** Suppose that the writer does not write a contract. Suppose that there is some selected equilibrium after no contract is written that has a partition $P = \{p_1, \ldots, p_n\}$ for $n \geq 1$. Let the partition elements be ordered based on the lowest state in each partition element, such that $s_1^1 < s_1^2 < \ldots < s_1^n$. Suppose that the writer had written the contract $C = \left(s^1_{low}, s^1_{high}, a^1_W\right)$ where $a^1_W$ is the writer preferred action given $s \in p_1$. Note that if there is a resulting communication subgame, $P^C = \{p_2, \ldots, p_n\}$ is a perfect Bayesian equilibrium of the communication subgame because all messages sent by states inside $p_1$ were unique, so there are still no incentives for the writer to change messages given that incentive constraints between the remaining partition elements have not changed. Therefore, there exists an equilibrium of the resulting communication subgame after $C$ that makes the writer strictly better off since $a^i_W - a^i_R > 0$ for $b \geq 1$. Thus writing a contract must be optimal. $\square$

## 3.8 Appendix C1: Calculating Optimal Contracts

In order to write down the optimal contract for any bias, I need to analyze every communication equilibrium in every possible communication subgame. Firstly, because of proposition 3, I can ignore the communication subgame after no contract is written since not writing a contract is never optimal for any $b$. Secondly, I can use proposition 1 to pin down $a_W = \frac{s_{low}+s_{high}}{2}$. In addition, given any partition $P$, I can use proposition 1 to pin down the receiver action that happens in that partition element. Using each of these properties, it remains to write down and compare the payoffs for any possible communication subgame to figure out what contract the writer will choose in period 1. Below, I will list all possible $n - state$ contracts, for $n \geq 1$, as well as the accompanying picture that shows which contract and communication subgame pair does better. After exhausting all possible communication subgames, I will compare the winners in each $n - state$ contract to get the optimal contract. As a further note, since $EU\left(C = \{1, 9, 5+b\}, P = \emptyset\right) = 26.16$, once payoffs dip below 26.16 the fully complete contract does better.

As another note, contracts that are symmetric around 5 can have equivalent sets of communication equilibria within communication subgames and thus equivalent payoffs. Thus only one of the two will have equilibria and payoffs presented.

$1 - state$ contracts:

$C = \{1, 1, 1+b\}$(symmetric to $C = \{9, 9, 9+b\}$):

($b \leq 1$) $P = \{\{3\}, \{5\}, \{7\}, \{9\}\}$: $EU_W\left(C, P\right) = 30 - \frac{4}{5}|b|^{1.4}$

($b \leq 1$) $P = \{\{3, 5\}, \{7, 9\}\}$: $EU_W\left(C, P\right) = 30 - \frac{2}{5}|b-1|^{1.4} - \frac{2}{5}|b+1|^{1.4}$

($b \leq 2$) $P = \{\{3\}, \{5, 7, 9\}\}$: $EU_W\left(C, P\right) = 30 - \frac{1}{5}|b-2|^{1.4} - \frac{2}{5}|b|^{1.4} - \frac{1}{5}|b+2|^{1.4}$

(all $b$) $P = \{\{3, 5, 7, 9\}\}$: $EU_W\left(C, P\right) = 30 - \frac{1}{5}|b-3|^{1.4} - \frac{1}{5}|b-1|^{1.4} - \frac{1}{5}|b+1|^{1.4} - \frac{1}{5}|b+1|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{3\}, \{5\}, \{7\}, \{9\}\} & b \leq 1 \\ P = \{\{3\}, \{5, 7, 9\}\} & 1 < b \leq 2 \\ P = \{\{3, 5, 7, 9\}\} & b > 2 \end{cases}$$

$C = \{3, 3, 3+b\}$(symmetric to $C = \{7, 7, 7+b\}$):

($b \leq 1$) $P = \{\{1\}, \{5\}, \{7\}, \{9\}\}$: $EU_W\left(C, P\right) = 30 - \frac{4}{5}|b|^{1.4}$

($b \leq 1.5$) $P = \{\{1\},\{5\},\{7,9\}\}$: $EU_W(C,P) = 30 - \frac{1}{5}|b-1|^{1.4} - \frac{2}{5}|b|^{1.4} - \frac{1}{5}|b+1|^{1.4}$

($b \leq 3$) $P = \{\{1\},\{5,7,9\}\}$: $EU_W(C,P) = 30 - \frac{1}{5}|b-2|^{1.4} - \frac{2}{5}|b|^{1.4} - \frac{1}{5}|b+2|^{1.4}$

(all $b$) $P = \{\{1,5,7,9\}\}$: $EU_W(C,P) = 30 - \frac{1}{5}|b-\frac{9}{2}|^{1.4} - \frac{1}{5}|b-\frac{1}{2}|^{1.4} - \frac{1}{5}|b+\frac{3}{2}|^{1.4} - \frac{1}{5}|b+\frac{7}{2}|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{1\},\{5\},\{7\},\{9\}\} & b \leq 1 \\ P = \{\{1\},\{5\},\{7,9\}\} & 1 < b \leq 1.5 \\ P = \{\{1\},\{5,7,9\}\} & 1.5 < b \leq 3 \\ P = \{\{1,5,7,9\}\} & b > 3 \end{cases}$$

$C = \{5, 5, 5+b\}$:

($b \leq 1$) $P = \{\{1\},\{3\},\{7\},\{9\}\}$: $EU_W(C,P) = 30 - \frac{4}{5}|b|^{1.4}$

($b \leq 2$) $P = \{\{1,3\},\{7,9\}\}$: $EU_W(C,P) = 30 - \frac{2}{5}|b-1|^{1.4} - \frac{2}{5}|b+1|^{1.4}$

($b \leq \frac{11}{3}$) $P = \{\{1\},\{3,7,9\}\}$: $EU_W(C,P) = 30 - \frac{1}{5}|b-\frac{10}{3}|^{1.4} - \frac{1}{5}|b|^{1.4} - \frac{1}{5}|b+\frac{2}{3}|^{1.4} - \frac{1}{5}|b+\frac{8}{3}|^{1.4}$

(all $b$) $P = \{\{1,3,7,9\}\}$: $EU_W(C,P) = 30 - \frac{1}{5}|b-4|^{1.4} - \frac{1}{5}|b-2|^{1.4} - \frac{1}{5}|b+2|^{1.4} - \frac{1}{5}|b+4|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{1\},\{3\},\{7\},\{9\}\} & b \leq 1 \\ P = \{\{1,3\},\{7,9\}\} & 1 < b \leq 2 \\ P = \{\{1\},\{3,7,9\}\} & 2 < b \leq \frac{11}{3} \\ P = \{\{1,3,7,9\}\} & b > 3 \end{cases}$$

Overall best $1 - state$ payouts:

$$\begin{cases} \text{Any contract with the most informative equilibrium} & b \leq 1 \\ C = \{3,3,3+b\}, P = \{\{1\},\{5\},\{7,9\}\} & 1 < b \leq 1.5 \\ C = \{5,5,5+b\}, P = \{\{1,3\},\{7,9\}\} & 1.5 < b \leq 2 \\ C = \{3,3,3+b\}, P = \{\{1\},\{5,7,9\}\} & 2 < b \leq 3 \\ C = \{1,1,1+b\}, P = \{\{3,5,7,9\}\} & b > 3 \end{cases}$$

$2 - \textit{state}$ contracts:

$C = \{1,3,2+b\}$(symmetric to $C = \{7,9,8+b\}$):

($b \leq 1$) $P = \{\{5\},\{7\},\{9\}\}$: $EU_W(C,P) = 29.6 - \frac{3}{5}|b|^{1.4}$

($b \leq 1.5$) $P = \{\{5\},\{7,9\}\}$: $EU_W(C,P) = 29.6 - \frac{1}{5}|b-1|^{1.4} - \frac{1}{5}|b|^{1.4} - \frac{1}{5}|b+1|^{1.4}$

(all $b$) $P = \{\{5,7,9\}\}$: $EU_W(C,P) = 29.6 - \frac{1}{5}|b-2|^{1.4} - \frac{1}{5}|b|^{1.4} - \frac{1}{5}|b+2|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{5\},\{7\},\{9\}\} & b \leq 1 \\ P = \{\{5\},\{7,9\}\} & 1 < b \leq 1.5 \\ P = \{\{5,7,9\}\} & b > 1.5 \end{cases}$$

$C = \{3,5,4+b\}$(symmetric to $C = \{5,7,6+b\}$):

($b \leq 1$) $P = \{\{1\},\{7\},\{9\}\}$: $EU_W(C,P) = 29.6 - \frac{3}{5}|b|^{1.4}$

($b \leq 3.5$) $P = \{\{1\},\{7,9\}\}$: $EU_W(C,P) = 29.6 - \frac{1}{5}|b-1|^{1.4} - \frac{1}{5}|b|^{1.4} - \frac{1}{5}|b+1|^{1.4}$

(all $b$) $P = \{\{1,7,9\}\}$: $EU_W(C,P) = 29.6 - \frac{1}{5}|b - \frac{14}{3}|^{1.4} - \frac{1}{5}|b + \frac{4}{3}|^{1.4} - \frac{1}{5}|b + \frac{10}{3}|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{1\},\{7\},\{9\}\} & b \leq 1 \\ P = \{\{1\},\{7,9\}\} & 1 < b \leq 3.5 \\ P = \{\{1,7,9\}\} & b > 3.5 \end{cases}$$

Overall best $2 - state$ payouts:

$$\begin{cases} \text{Any contract with the most informative equilibrium} & b \leq 1 \\ C = \{1,3,2+b\}, P = \{\{5\},\{7,9\}\} & 1 < b \leq 1.5 \\ C = \{3,5,4+b\}, P = \{\{1\},\{7,9\}\} & 1 < b \leq 3.5 \\ C = \{1,3,2+b\}, P = \{\{5,7,9\}\} & b > 3.5 \end{cases}$$

$3 - state$ payouts:

$C = \{1,5,3+b\}$(symmetric to $C = \{5,9,7+b\}$):

$(b \leq 1)$ $P = \{\{7\},\{9\}\}$: $EU_W(C,P) = 28.944 - \frac{2}{5}|b|^{1.4}$

$(b \geq 1)$ $P = \{\{7,9\}\}$: $EU_W(C,P) = 28.944 - \frac{1}{5}|b-1|^{1.4} - \frac{1}{5}|b+1|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{7\},\{9\}\} & b \leq 1 \\ P = \{\{7,9\}\} & b > 1 \end{cases}$$

$C = \{3,7,5+b\}$:

$(b \leq 4)$ $P = \{\{1\},\{9\}\}$: $EU_W(C,P) = 28.944 - \frac{2}{5}|b|^{1.4}$

$(b \geq 4)$ $P = \{\{1,9\}\}$: $EU_W(C,P) = 28.944 - \frac{1}{5}|b-4|^{1.4} - \frac{1}{5}|b+4|^{1.4}$

Best equilibria:

$$\begin{cases} P = \{\{1\},\{9\}\} & b \leq 4 \\ P = \{\{1,9\}\} & b > 4 \end{cases}$$

Overall best $3 - state$ payouts:

$$\begin{cases} C = \{1, 5, 3 + b\}, P = \{\{7\}, \{9\}\} & b \leq 1 \\ C = \{3, 7, 5 + b\}, P = \{\{1\}, \{9\}\} & b \leq 4 \\ C = \{1, 5, 3 + b\}, P = \{\{7, 9\}\} & b > 4 \end{cases}$$

$4 - state$ contracts:

$C = \{1, 7, 4 + b\}$(symmetric to $C = \{3, 9, 6 + b\}$):

(all $b$) $P = \emptyset$: $EU_W(C, P) = 27.738 - |b|^{1.4}$

$5 - state$ contract:

(all $b$) $P = \emptyset$: $EU_W(C, P) = 26.159$

Overall, the best contracts are detailed in table 1.1 when comparing among the best among each $n - state$ contract. This is shown graphically here in figure 3.17. Each line represents a piecewise function as described above.

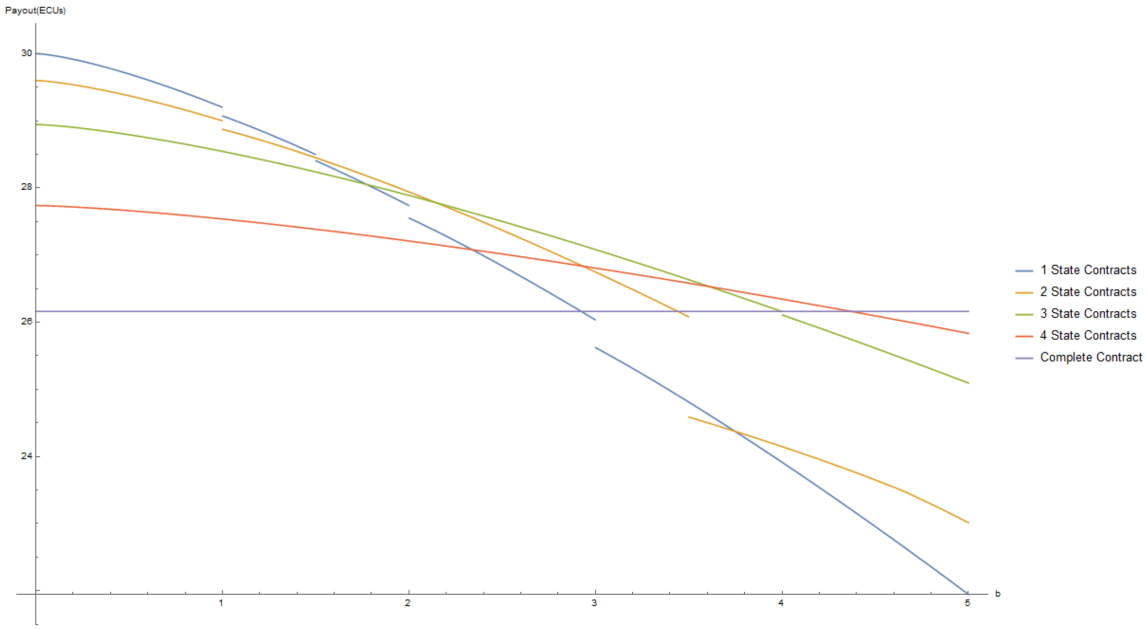



Figure 3.17: Comparison between all best $n - state$ contract/equilibrium partition pairs

## 3.9 Appendix D1: Instructions

Below are the instructions for the US $b = 1.25$ treatment:

Welcome! In this experiment, your earnings will depend on your choices, the choices of others, and chance. Please refrain from talking to others until the experiment has concluded. In addition, please silence and put away any electronic devices (although listening to music is allowed).

The participants of this experiment will be randomly split between Writers and Receivers, such that half will be Writers and the other half will be Receivers. You will play only as a Writer or as a Receiver for the duration of the experiment. In each round, each Writer will be randomly paired with a Receiver. You will not see the identity of the person you are paired with, but you will see each player's decisions at the end of each round. Your total payment at the end of the experiment will be the sum of your earnings across the 9 paid rounds of the game.

Your payment in each round, in Experimental Currency Units (ECUs), depends on a randomly drawn state and on choices both players will make that dictate an action for each of those states. This action is decided in part by the Writer, who moves first, and in part by the Receiver, who moves second after observing the Writer's choices. The details of this process will be described below.

States: There are 5 random states that can occur, numbered 1, 3, 5, 7, and 9. The states that occur in this experiment will be computer generated and all states will be equally likely in each round. There will be a state drawn after the Writer has written the Writer's rule.

Writer's Rule: In this experiment, the Writer will be writing a rule. This rule will indicate a 'low state,' a 'high state,' and a 'rule action.' The 'low state' can be any state (1, 3, 5, 7, or 9). The 'high state' can be any state (1, 3, 5, 7, or 9) that is higher than or equal to the 'low state.' The 'rule action' can be any action from 1 to 12 that is a multiple of .25. This rule will help to determine the action that is taken. If a state is drawn that is between 'low state' and 'high state', or equal to either of these states, the rule will dictate that the 'rule action' is taken. The Writer can also choose not to write a rule. The rule that the Writer writes is shown to the Receiver.

State Draw and Message Sending: After the Writer writes his/her rule, the state will be drawn. If the state that is drawn is between 'low state' and 'high state' or equal to either of those states, the 'rule action' is taken and the round will end. If a state is drawn that is below 'low state' or above 'high state,' or no rule was written, only the Writer will observe the state. The Writer, after observing the state, must send a message to the Receiver, who will see the message and then take a 'Receiver action'. The message that the Writer can send can be one of the following numbers: 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

Receiver's Action: After the Writer writes his/her rule, the Receiver may receive a message. If the Receiver receives a message, the Receiver will observe only the message and the Writer's rule and then take a 'Receiver action.' The 'Receiver action' can be any action between 1 and 12 that is a multiple of .25. The Receiver does not observe the state when they choose an action.

In summary, each round of the experiment will be as follows:

First: The Writer can either write a rule or not write a rule. A rule indicates three things: 'low state', 'high state', and 'rule action.' The 'high state' must be a state with number higher than or equal to the number the Writer writes down for 'low state.' The 'rule action' can be any action from 1 to 12 that is a multiple of .25.

Second: Then, after the Writer writes (or does not write) a rule, the state will be drawn. If a rule has been written and the state is between 'low state' and 'high state' or equal to 'low state' or 'high state,' the computer will take the 'rule action' and the round will end. Otherwise, if the state drawn is less than 'low state' or higher than 'high state,' or no rule was written, the Writer will privately observe the state and then send a message to the Receiver. The possible messages are 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

Third: If the Receiver receives a message, the Receiver observes the message and the rule and then takes a 'Receiver action.' The 'Receiver action' can be any action from 1 to 12 that is a multiple of .25.

At the end of each round, you will be shown the decisions of both you and your partner, the action taken, and your earnings for the round.

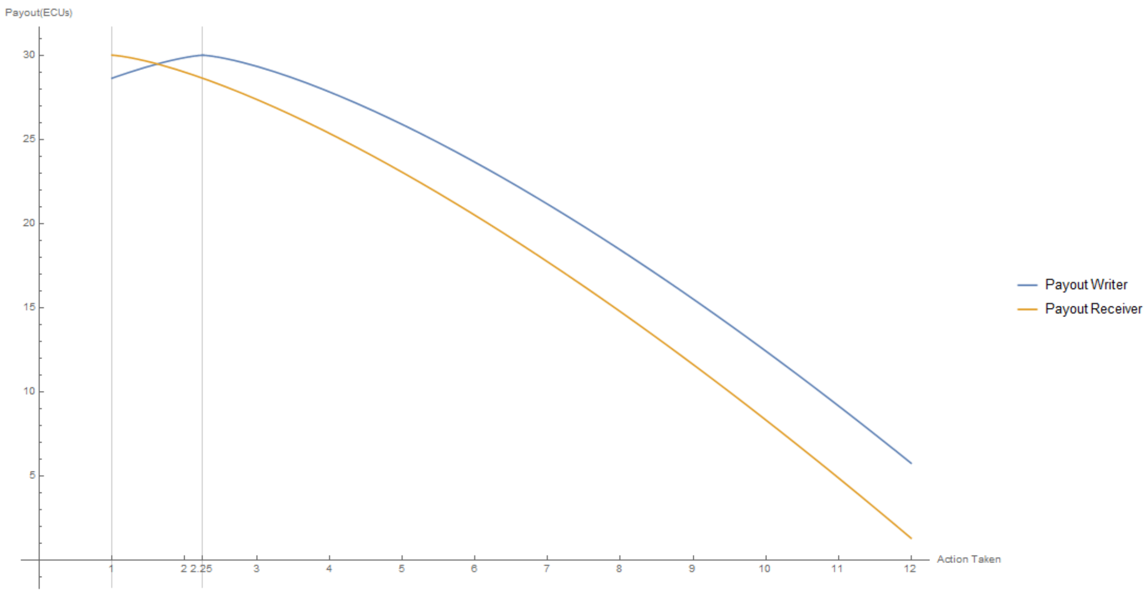
Your payout, depending on the action and state, is detailed graphically on the next page. It is decided using the following formula:

Writer's Payout = $30- \mid state + 1.25 - action\ taken \mid^{1.4}$.

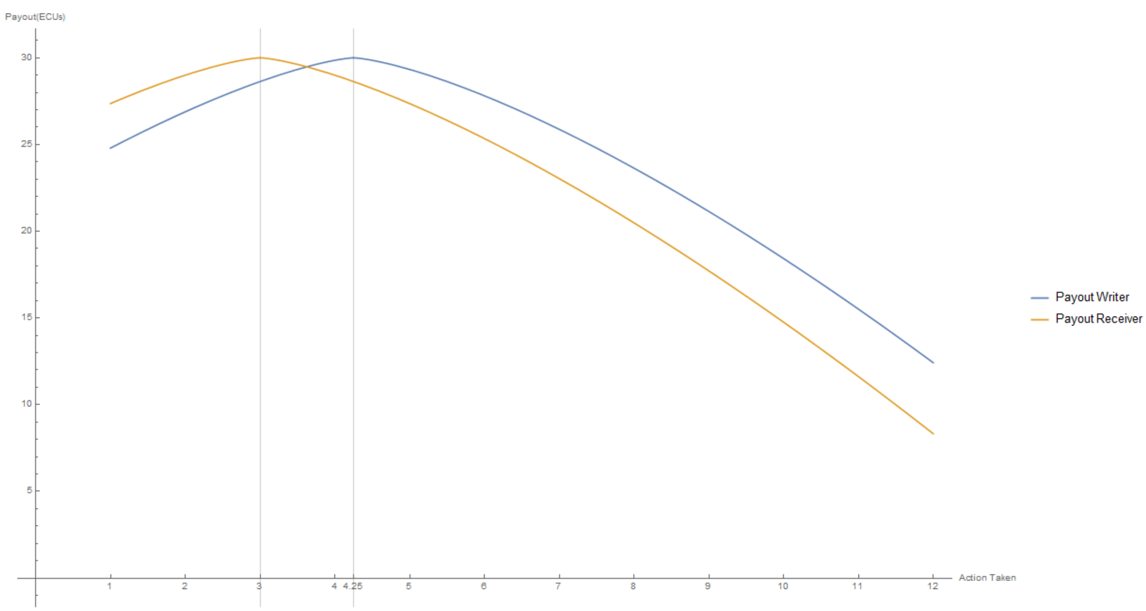
Receiver's Payout = $30- \mid state - action\ taken \mid^{1.4}$

Verbally, the Writer's payout is: take the absolute difference between the state plus 1.25 and the action taken and raise that number to the power of 1.4; then subtract that number from 30. The Receiver's Payout is: take the absolute difference between the state and the action taken and raise that number to the power of 1.4; then subtract that number from 30. This payout is displayed graphically on pages 5-7 for each state. Note that the action that gives the Writer and the Receiver the highest payout in each state is indicated by a line. You will also be able to see your payout on sliders in the experiment itself. The sliders allow you to adjust the action in each state to see possible payouts. The sliders will be on the left side of the screen at any point when you are not in a waiting screen. (Note: The sliders can lag a bit, so be careful that you have the correct number selected)

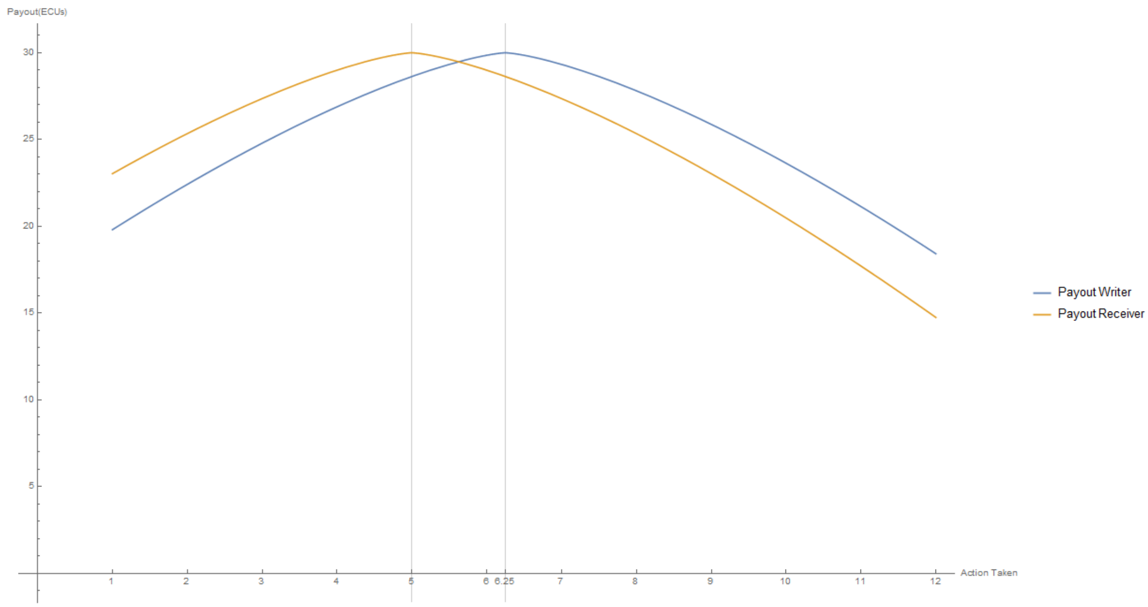
You will play 2 practice rounds of the task by yourself as both the Writer and the Receiver where you will be quizzed on the things that happen in those trials at the end of each trial period. After that, the task will be repeated 40 times, with random matching in each round, and where your role will stay fixed as either the Writer or the Receiver. Your total earnings from this experiment will be your earnings from 2 of the 40 periods, drawn randomly by you at the end of the experiment, plus your show up fee of $6. The payments in each period will be recorded in Experimental Currency Units (ECUs). Each ECU is worth 33 cents (.33 dollars).



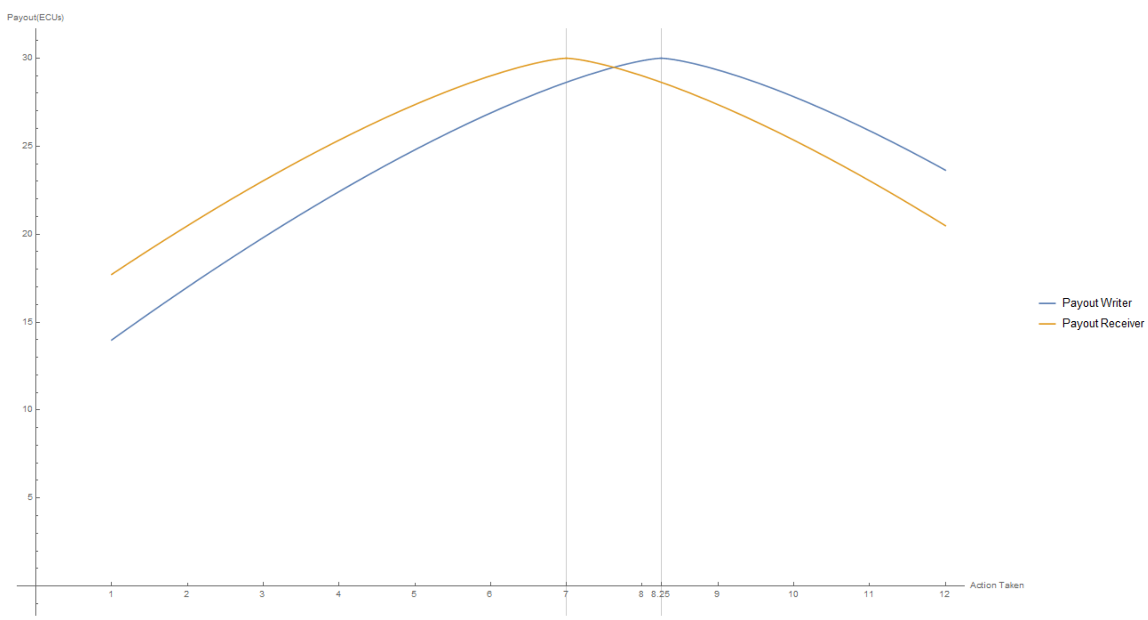

State=1 Payouts



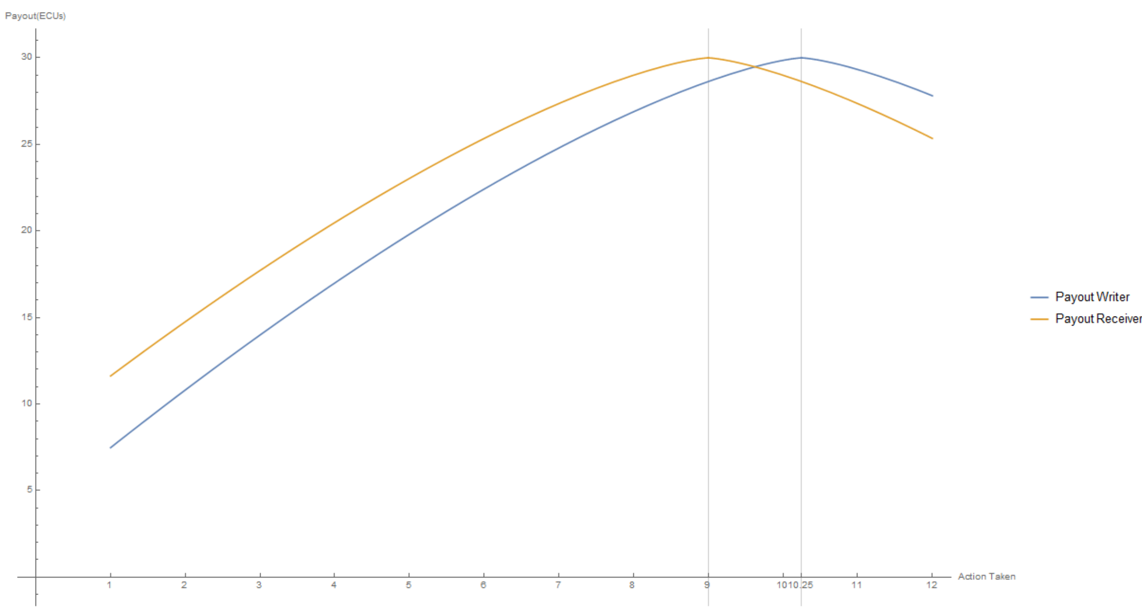

State=3 Payouts




State=5 Payouts




State=7 Payouts




State=9 Payouts

# Appendix for Chapter 2

## 3.10 Appendix A2: Figures of Aggregate Play

Figure 3.18: Messages Sent in Communication Treatment

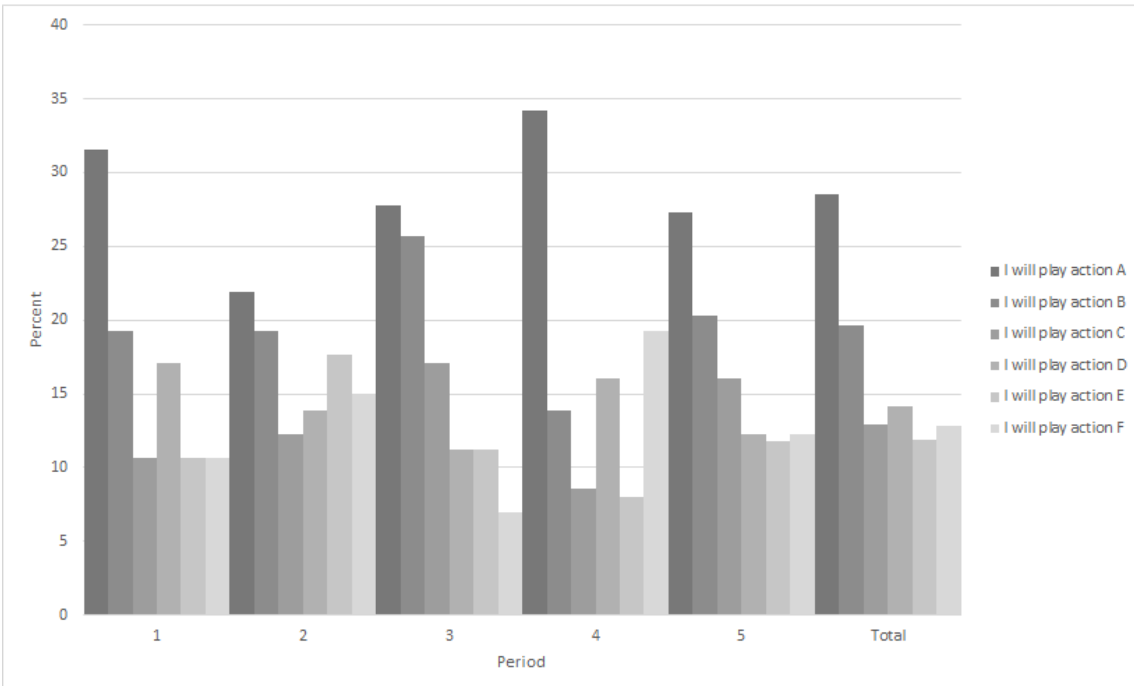


Figure 3.19: Sender Actions with Communication

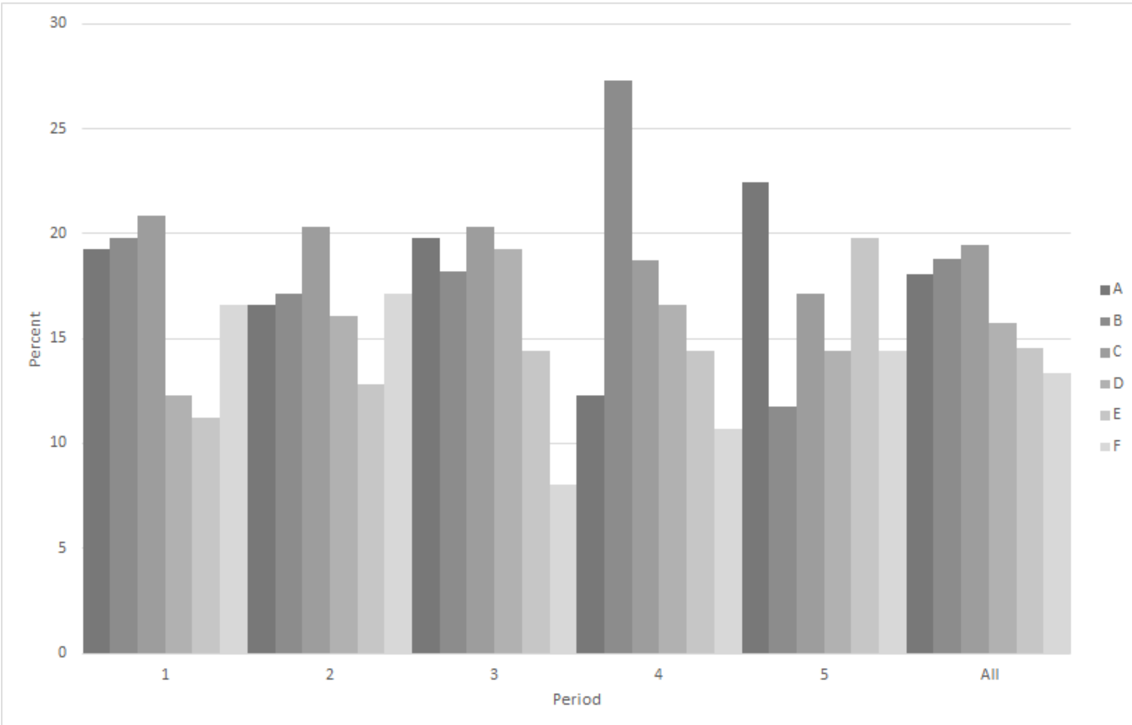


Figure 3.20: Receiver Actions with Communication

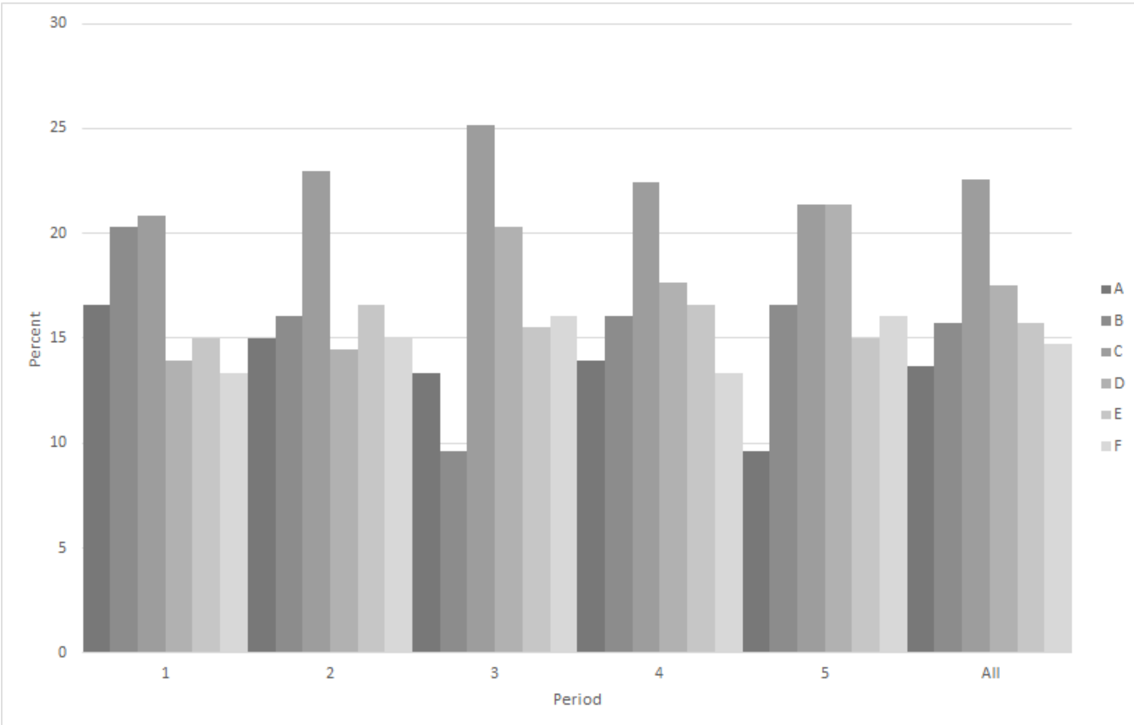


Figure 3.21: Sender Levels with Communication

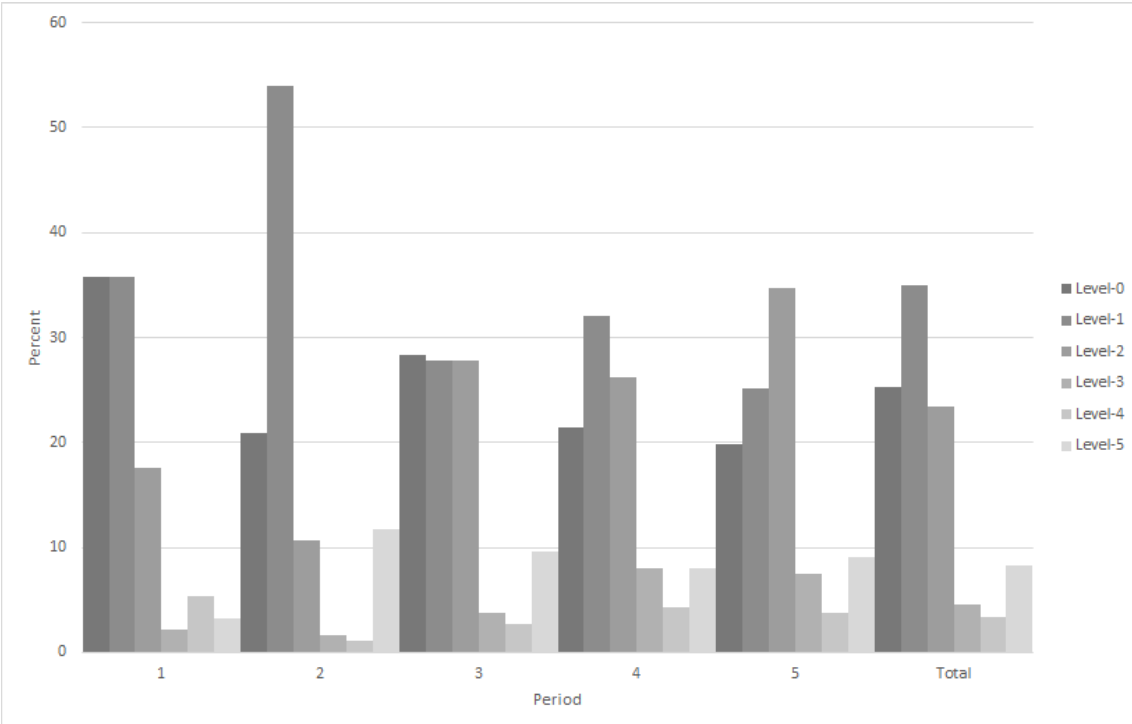
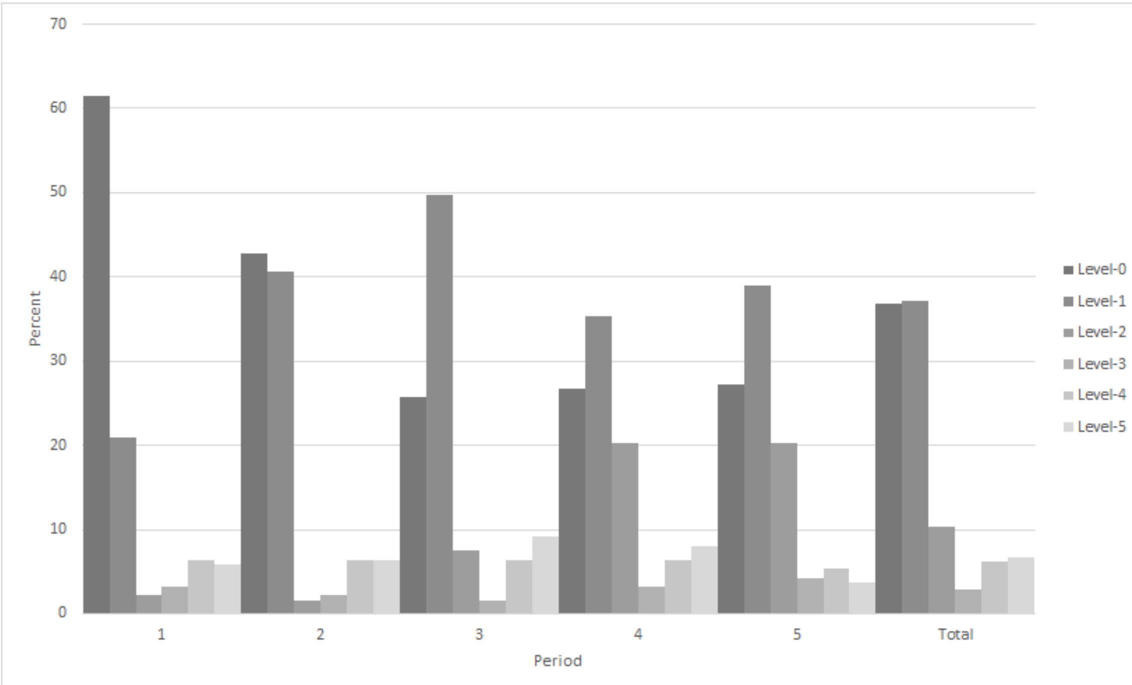


Figure 3.22: Receiver Levels with Communication




## 3.11 Appendix B2: Instructions

# Instructions A

Welcome! In this experiment your earnings will be based on the decisions of yourself and other players as well as chance. Please refrain from talking to others during the experiment. In addition, please silence and refrain from using any electronic devices while the experiment is ongoing. If you have any questions or concerns, please raise your hand.

In this experiment, you will be either a Sender or a Receiver. You have an equal chance of being a Sender or a Receiver. Senders will be randomly paired with Receivers in each round of the experiment. You will not see the identity of the person you are paired with, but you will see each player's decisions at the end of each period.

In each round, you and your partner will play the game depicted on the last page of the instructions. Payoffs are in Experimental Currency Units (ECUs). Sender actions and payoffs are in blue, while Receiver actions and payoffs are in red. Both players will pick their actions simultaneously. Note that both the Sender and the Receiver have 6 possible actions, labeled by the following six symbols: #, %, ^, +, * , and (. The payoffs of each player depend on the actions of both players. The Sender's payoff is the first number in each box, while the Receiver's payoff is the second number in each box. Note that the only possible payoff a player can earn is 1 or 0. Also note that the Sender receives a payoff of 1 when the actions of both players match. The Receiver receives a payoff of 1 when the Sender plays # and the Receiver plays %, when the Sender plays % and the Receiver plays ^, when the Sender plays ^ and the Receiver plays +, when the Sender plays + and the Receiver plays *, when the Sender plays * and the Receiver plays (, and when the Sender plays ( and the Receiver plays #.

Prior to playing the game, the Sender will send one of the following messages to the Receiver: I will take action #, I will take action %, I will take action ^, I will take action +, I will take action *, or I will take action (. The message will be observed by the Receiver before both players simultaneously choose their actions.

In summary, each round will proceed as follows:

**First:** The Sender sends a message to the Receiver. The message is one of the following: I will take action #, I will take action %, I will take action ^, I will take action +, I will take action *, or I will take action (.

**Second:** Both the Sender and Receiver will simultaneously take an action. An action can be either #, %, ^, +, * , and (. Payoffs for each combination of actions are listed in the figure on the next page.

You will play a total of 5 periods of this experiment. In each period, Senders will be randomly matched and play a round with every Receiver, and Receivers will be randomly matched and play a round with every Sender. In each period, each subject will decide on all actions simultaneously. This means that first, messages will be selected by Senders for every Receiver in the room. Following this, Receivers will observe the messages sent to them and both Senders and Receivers will make all decisions. The identity of each partner will be anonymous and randomized.

At the end of the experiment, each subject will be paid for one **round**, which will be randomly determined by each subject at the end of the experiment. Each subject will be paid $8 for each ECU earned, as well as a $6 appearance fee. Additional questions will be asked at the end of the experiment, which will earn $1 for each correct answer.

| | | Receiver | | | | | |
|---|---|---|---|---|---|---|---|
| | | # | % | ^ | + | * | ( |
| Sender | # | 1, 0 | 0, 1 | 0, 0 | 0, 0 | 0, 0 | 0, 0 |
| | % | 0, 0 | 1, 0 | 0, 1 | 0, 0 | 0, 0 | 0, 0 |
| | ^ | 0, 0 | 0, 0 | 1, 0 | 0, 1 | 0, 0 | 0, 0 |
| | + | 0, 0 | 0, 0 | 0, 0 | 1, 0 | 0, 1 | 0, 0 |
| | * | 0, 0 | 0, 0 | 0, 0 | 0, 0 | 1, 0 | 0, 1 |
| | ( | 0, 1 | 0, 0 | 0, 0 | 0, 0 | 0, 0 | 1, 0 |

# Appendix for Chapter 3

## 3.12 Appendix A3: Instructions

The following are the Instructions for the Wfirst, b=2 treatment:

Instructions C

Welcome! In this experiment, your earnings will depend on your choices, the choices of others, and chance. Please refrain from talking to others until the experiment has concluded. In addition, please silence and put away any electronic devices.

The participants of this experiment will be randomly split between Writers and Deciders, such that half will be Writers and the other half will be Deciders. You will play only as a Writer or as a Decider for the duration of the experiment. In each round, each Writer will be randomly paired with a Decider. You will not see the identity of the person you are paired with, but you will see each player's decisions at the end of each round.

Your payment in each round, in Experimental Currency Units (ECUs), depends on a randomly drawn state and on choices both players will make that dictate an action for each of those states. This action is decided in part by the Writer, who moves first, and in part by the Decider, who moves second after observing the Writer's choices. The details of this process will be described below.

**States:** There are 6 random states that can occur, numbered 1, 2, 3, 4, 5, and 6. The states that occur in this experiment will be computer generated and **all states will be equally likely** in each round. There will be a state drawn after the Writer and the Decider have moved at the end of each round.

**Writer's Rule:** In this experiment, the Writer will be writing a rule. This rule will indicate a 'low state,' a 'high state,' and a 'rule action.' The 'low state' can be any state (1 through 6). The 'high state' can be any state (1 through 6) that is higher than or equal to the 'low state.' The 'rule action' can be any action from 2 to 14. This rule will help to determine the action that is taken. **If a state is drawn that is between 'low state' and 'high state', or equal to either of these**

**states, the rule will dictate that the 'rule action' is taken.**

**Decider's Decision:** After the Writer writes his/her rule, the Decider will observe this and then decide on a 'default action,' which can be any action between 2 and 14. **In the case that the state that is drawn at the end of the period is either lower than the 'low state' or higher than the 'high state' specified in the Writer's rule, the 'default action' will be taken.**

In summary, each round of the experiment will be as follows:

**First**: The Writer will write a rule that indicates three things: 'low state', 'high state', and 'rule action.' The 'high state' must be a state with number higher than or equal to the number the Writer writes down for 'low state.' The 'rule action' can be any action from 2 to 14.

**Second**: Then, after the Writer writes a rule, the Decider will decide on a 'default action.' This action will be the 'default action' taken in the case that the state drawn is lower than 'low state' or higher than 'high state'. The 'default action' can be any action from 2 to 14.

**Third**: After the Decider decides on a 'default action,' the state will be drawn. The 'rule action' will be taken if the state is between 'low state' and 'high state' or at 'low state' or 'high state.' Otherwise, if the state drawn is less than 'low state' or higher than 'high state,' the 'default action' will be taken.

At the end of each round, you will be shown the decisions of both you and your partner for the period, the action taken, and your earnings for the round. Your payout, depending on the action and state, is detailed on the next page.

You will play 2 practice rounds of the task by yourself as both the Writer and the Decider where you will be quizzed on the things that happen in those trials at the end of each trial period. After that, the task will be repeated 15 times, with random matching in each round, and where your role will stay fixed as either the Writer or the Decider. **Your total earnings from this experiment will be your earnings from 2 of the 15 periods, drawn randomly by you at the end of the experiment, plus your show up fee of $6.** The payments in each period will be recorded in Experimental Currency Units (ECUs). **Each ECU is worth 30 cents (.3 dollars).**

**The Writer's payout (in ECUs) followed by The Decider's payout (in ECUs)** is presented below for each action and state:

| *Payouts* | | *State* | | | | | |
|---|---|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 | 5 | 6 |
| | 2 | (20, 18) | (18, 16) | (16, 14) | (14, 12) | (12, 10) | (10, 8) |
| | 3 | (19, 19) | (19, 17) | (17, 15) | (15, 13) | (13, 11) | (11, 9) |
| | 4 | (18, 20) | (20, 18) | (18, 16) | (16, 14) | (14, 12) | (12, 10) |
| | 5 | (17, 19) | (19, 19) | (19, 17) | (17, 15) | (15, 13) | (13, 11) |
| | 6 | (16, 18) | (18, 20) | (20, 18) | (18, 16) | (16, 14) | (14, 12) |
| | 7 | (15, 17) | (17, 19) | (19, 19) | (19, 17) | (17, 15) | (15, 13) |
| *Action* | 8 | (14, 16) | (16, 18) | (18, 20) | (20, 18) | (18, 16) | (16, 14) |
| | 9 | (13, 15) | (15, 17) | (17, 19) | (19, 19) | (19, 17) | (17, 15) |
| | 10 | (12, 14) | (14, 16) | (16, 18) | (18, 20) | (20, 18) | (18, 16) |
| | 11 | (11, 13) | (13, 15) | (15, 17) | (17, 19) | (19, 19) | (19, 17) |
| | 12 | (10, 12) | (12, 14) | (14, 16) | (16, 18) | (18, 20) | (20, 18) |
| | 13 | (9, 11) | (11, 13) | (13, 15) | (15, 17) | (17, 19) | (19, 19) |
| | 14 | (8, 10) | (10, 12) | (12, 14) | (14, 16) | (16, 18) | (18, 20) |

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